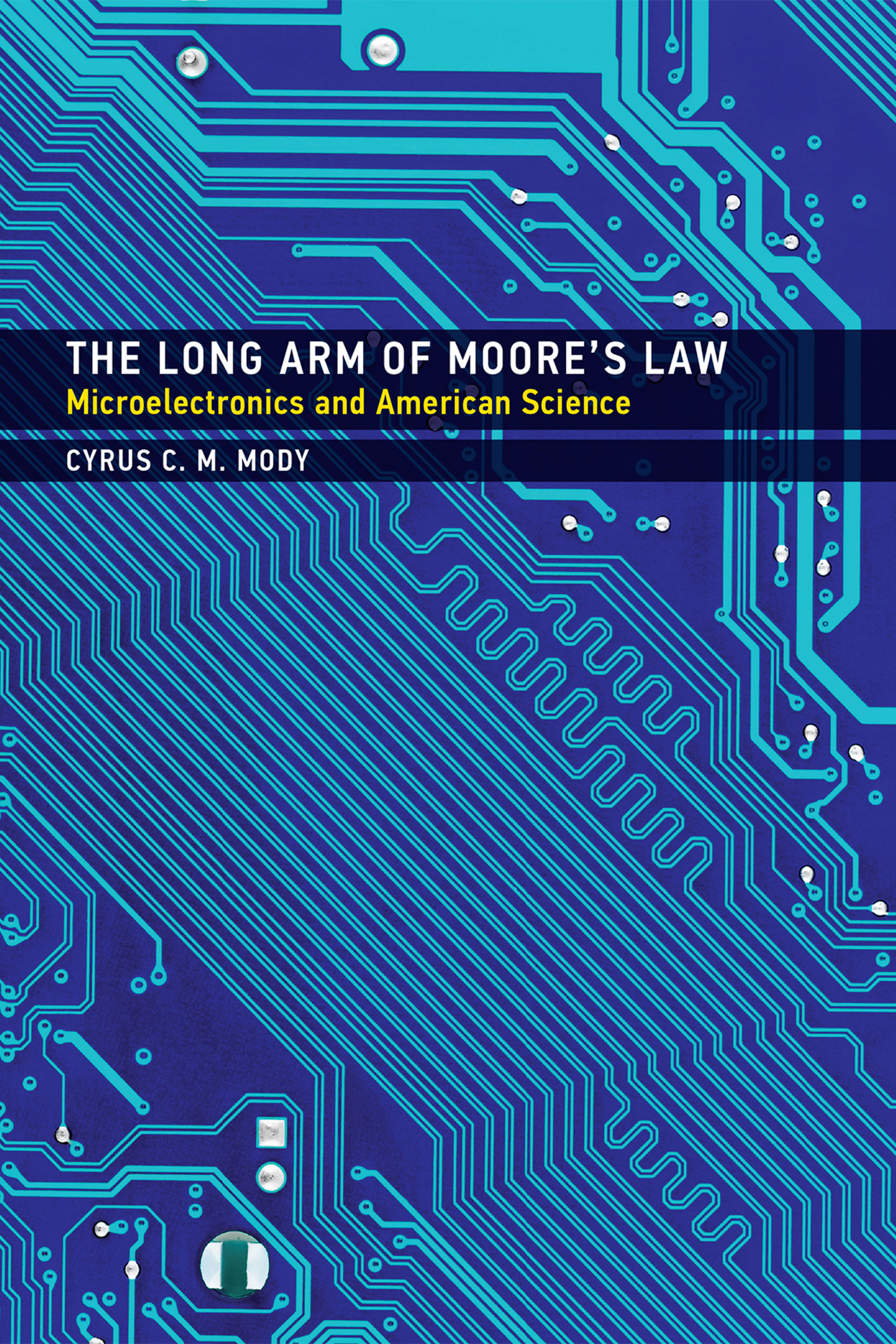


THE LONG ARM OF MOORE'S LAW

Microelectronics and American Science

CYRUS C. M. MODY

The Long Arm of Moore's Law

Inside Technology

edited by Wiebe E. Bijker, W. Bernard Carlson, and Trevor Pinch

A list of the series appears at the back of the book.

The Long Arm of Moore's Law

Microelectronics and American Science

Cyrus C. M. Mody

The MIT Press
Cambridge, Massachusetts
London, England

© 2017 Massachusetts Institute of Technology

All rights reserved. No part of this book may be reproduced in any form by any electronic or mechanical means (including photocopying, recording, or information storage and retrieval) without permission in writing from the publisher.

Set in Stone Serif and Stone Sans by Toppan Best-set Premedia Limited. Printed and bound in the United States of America.

Library of Congress Cataloging-in-Publication Data

Names: Mody, Cyrus C. M. (Cyrus Cawas Maneck), 1974- author.
Title: The long arm of Moore's law : microelectronics and American science / Cyrus C.M. Mody.
Description: Cambridge, MA : The MIT Press, [2017] | Series: Inside technology | Includes bibliographical references and index.
Identifiers: LCCN 2016020051 | ISBN 9780262035491 (hardcover : alk. paper)
Subjects: LCSH: Microelectronics--Research--United States--History. | Moore's law. | Science--United States--History.
Classification: LCC TK7874 .M532525 2017 | DDC 621.381--dc 3 LC record available at https://lccn.loc.gov/2016020051

10 9 8 7 6 5 4 3 2 1

for Karen Cassandra Burk

Contents

Preface ix

Introduction 1
1 Crisis and Opportunity at Vietnam-Era Stanford 25
2 IBM Gives Josephson Computing a Try 47
3 Molecular Electronics Gains a Reputation 79
4 New Institutions for Submicron Research 119
5 Circuits, Cells, and Networks 149
6 Synthesis at the Center 185
Epilogue 221

Notes 227
Index 279

Preface

Many people contributed to this book, and those not listed here should know they have my thanks. A few individuals and organizations, however, merit mention for leaving their marks on specific sections of the manuscript. To begin with the title, the "Long Arm of Moore's Law" tag line and a rudimentary version of the introduction were fashioned for a seminar at Amherst College to which Javier Lezaun and Susan Silbey suggested I be invited. Javier also got me into a workshop on interdisciplinarity at Oxford at which I presented parts of chapter 1, on which Sharon Traweek, Natasha Myers, and Steve Hilgartner gave much helpful feedback. The themes of that chapter first germinated in an essay review commissioned by Michael Gordin and Angela Creager and in a chapter for a handbook edited by Karin Bijsterveld and Trevor Pinch. The chapter also benefited from Christophe Lécuyer's advice and collaborations with Andrew Nelson and Mara Mills.

Chapter 2 began as a loose thread from my first book project, which I was able to follow thanks to help from Alan Kleinsasser. That chapter, and everything else I write, were further shaped by conversations with David Brock and Ann Johnson. Randal Hall and the members of the University of Houston Center for Public History discussion group also helped me refine the chapter.

Chapters 3–6 would not have been possible without Hyungsub Choi and our decade of collaboration. Arthur Daemmrich inspired chapter 4 when he suggested I look at the Gordon Research Conference archives that the Chemical Heritage Foundation had recently acquired. Later, Hyungsub and CHF provided funding for a preliminary version of parts of chapter 6 through the Studies in Materials Innovation program. Chapters 3–6 were also improved by a summer at Bielefeld University's Center for Interdisciplinary Research, especially by conversations that summer with Alfred Nordmann, Hans Glimell, and Johannes Lenhard. Chapters 4 and 5 read better, I hope, thanks to refinements for conferences organized by Thomas Heinze and Richard Münch (chapter 4) and by Anna Guagnini, Joris Mercelis, and their collaborators (chapter 5).

Chapter 6 draws on interactions with Rice University colleagues, especially Kevin Kelly and Kristen Kulinowski, and on support (financial and otherwise) from Rice's Smalley-Curl Institute, Woodson Research Center, and Center for Biological and Environmental Nanotechnology (CBEN). Chapters 4–6 incorporate research funded by the National Nanotechnology Infrastructure Network (NNIN) and the Center for Nanotechnology in Society (CNS) at UC Santa Barbara. The National Science Foundation was the primary funder of CBEN, NNIN, and CNS; I also received a Scholars Award directly from NSF that enabled a year of teaching leave. Mike Gorman's oversight of that award and of CNS, and his championing of "nano in society" research, helped make this project possible. I took another semester of leave thanks to an American Council of Learned Societies Collaborative Research Fellowship, in partnership with Mara, Hyungsub, and Patrick McCray. Patrick has been a booster, colleague, and friend throughout our decade of participation in CNS.

Finally, I'm proud to thank those who created the environment in which this book was written. Lora Wildenthal, as chair of Rice's History Department, gets special thanks for encouragement to apply for the grants listed above. Colleagues in history of science and technology, nano studies, innovation studies, and science and technology studies have been a joy to read, write for, and work with—including, among many others, Amy Slaton, Terry Shinn, Sonali Shah, Chris Toumey, Sarah Kaplan, Matt Eisler, and Jody Roberts. The Mody and Inkeles families gave love and perspective. Most of all, Karen Burk and Daria Mody are why I could write this at all.

Introduction

The manuscript of this book was written on a computer that was itself networked to billions of other computers and computer-like devices all over the world. I need not enumerate the capabilities of today's computers, since virtually everyone reading this book knows them. They offer unprecedented access to information and misinformation; they boost our productivity and allow us to waste time in new ways; they keep us in touch and they keep us apart.

More germane to this study is that the computer and electronics industry contributes more to the United States' gross domestic product than any other manufacturing industry.[1] Worldwide, electronics manufacturing is a prized source of revenue, employment, and prestige for industrialized and industrializing societies alike.[2] It has been a major factor in the rapid growth of the "Asian Tiger" economies since the 1970s, and in the globalization (more accurate, re-globalization) of the world economy in the late twentieth century.[3] For those reasons, many governments have gone to great lengths to acquire or to maintain a thriving computer and/or electronics industry.

With some exceptions, the first digital electronic computers were products of the military-industrial complex that arose during World War II and the Cold War.[4] Digital computers quickly found civilian applications, but early in the Cold War the US national-security state was the major funder of, and the major market for, commercial development of digital computers—especially the biggest, fastest, most complex, most networked computers. The same goes for advanced microelectronics in general, not just computers. In particular, silicon transistors and integrated circuits were originally developed for military applications such as missile guidance systems, and only later became standard for digital computers. Transistors, integrated circuits, and computers all required both hands-on empirical know-how and fundamental, theory-driven discovery.[5] Accordingly, national-security agencies weren't just the major buyers for most early computers and microelectronic devices; they were also the most important funders and shapers of the basic *and* applied research needed to advance computing and microelectronics.[6]

Today the US military and other national-security agencies are still important customers for computers and microelectronics, and their research funding arms are still indispensable to innovation in those technologies. But the center of gravity of computing and microelectronics has shifted dramatically and decisively away from national-security customers to civilian—indeed, consumer—markets. Today, military needs are often met by adapting consumer products such as laptop computers and video game consoles, whereas in the early Cold War the arrow more commonly ran from military to civilian domains.[7] This civilianization of the computer and electronics industry had many roots, including countercultural ideas about "personal" computing,[8] microelectronics firms (grown wealthy from military contracts in the 1950s) coming to see government as an unreliable customer in the 1960s,[9] and increasing competition between US firms and their Japanese counterparts (which had access to a large domestic consumer market but a much smaller military market).[10]

The basic premise of this book is that the civilianization of computing and microelectronics since the mid 1960s co-evolved with large-scale changes in the ways American science is organized, funded, and practiced. Those changes dismantled relationships among universities, corporations, and the US government that were established in the early Cold War—the era of national security dominance of computing—and reconstructed a new set of relationships that continues to today. The rough outlines of these changes in American science are well documented because scientists, engineers, politicians, economists, sociologists, and cultural critics have argued about them for fifty years.[11] Indeed, the scholarly debate today is not about *whether* science has changed significantly since the late 1960s, but about whether that change has been so dramatic and sudden as to constitute an "epochal break" in which scientific truth ceded primacy to technological instrumentality.[12]

The characteristics of the purported epochal break that receive most comment, and that occupy this book, include the decline of corporate basic research,[13] the shortening of the time span in which research is expected to find technological application or have "broader societal impacts,"[14] and increasing pressure on universities and government laboratories to participate in the marketplace by entering into partnerships with industry and by converting research into intellectual property.[15] Some less publicized changes also figure here, including the federal government's retreat from funding construction of university buildings, the growing interest in institutions designed to mediate interactions among academic disciplines or between universities and outside stakeholders, greater oversight and regulation of how academic and corporate research is organized and practiced, reinterpretation of antitrust laws to promote research and development consortia, and increasing emphasis on maximum, auditable return on public and private investment in research (for example, through the sharing of expensive experimental apparatus).[16]

Although there is some consensus on what has changed in American science in the last fifty years, there is considerable, sometimes vitriolic disagreement about why those changes have occurred. Some scholars, notably Philip Mirowski, see them as having been driven primarily by neoliberal economists and by allied market-friendly politicians of both major US political parties.[17] On this view, neoliberal science policy is self-defeating because it eats the seed corn of basic research and doesn't replenish it. Other scholars, including David Mowery and Nathan Rosenberg, point to an increasingly competitive environment for American companies (shortening the time for research to yield products) and universities (leading to more need to demonstrate societal benefits of academic research and to secure funding from private sources).[18] Mowery and his co-authors have documented that the pace of academic patenting and start-up formation increased around 1970, seemingly without much aid from neoliberal policies.[19] Mowery and Martin Kenney also argue that universities that partner with firms often receive goods (people, materials, ideas, etc.) that boost their ability to do both applied and (*pace* Mirowski) basic research.[20] Despite the disagreements between the Mirowski and Mowery camps, this study incorporates many insights from both scholars and from their collaborators.

But economic history on its own can't fully explain why and how American science has changed since the late 1960s. We will see, for instance, that idealistic campus activism and the cultural disruptions of the Southeast Asian conflict promoted interdisciplinarity, civilian applications, and entrepreneurship at American universities around 1970.[21] Other not entirely economic trends also were involved, among them professionalization of university patent officers, deceleration of the space and arms races, and an increase in prestige and funding for the life sciences relative to the physical sciences.[22] In a more sinister vein, moral panic about a resurgent Japan stimulated calls for reform in American science policy in the late 1970s and the 1980s, as did charismatic fabulists touting visions of libertarian techno-utopias.[23]

Though scholars disagree on why American science became more commercial and more oriented toward the short term, there is more consensus about which fields and industries have led those changes. Most observers assign the lion's share of credit or blame to the biotechnology industry for reshaping US patent law and turning high-tech start-ups into revered icons of economic progress.[24] This book undermines that consensus, however, by highlighting the role of the microelectronics industry and its allied disciplines in fomenting shifts in science policy and in scientific practice. To be sure, the actors in this story knew what was happening in the life sciences. Yet they saw microelectronics as equally central to the changes that were sweeping across American science. As the magazine *Science* put it in 1982,

> While attention has been focused on the expanding links between academic biologists and the corporate world, a second revolution in university-industry relationships has been taking place in a different field. Electronics companies, faced with growing competition from Japan and fearing a shortage of well-trained Ph.D.s, are pouring unprecedented amounts of cash into university electrical engineering and computer science departments.[25]

The life sciences should be understood as just one among many mutually interdependent niches within American science that began re-organizing in the late 1960s. Domains of American science that could form a plausible connection to the microelectronics industry re-organized at roughly the same time, sometimes following the model of domains with connections to the biotechnology and pharmaceutical industries but sometimes racing ahead to create their own model.

Moore's Law as Social Fact

Microelectronics firms fostered the re-organization of American science because of their constant need for innovation. By "innovation" I mean new products and new manufacturing processes as well as the new knowledge and techniques enabling those products and processes. But my understanding of innovation also encompasses new institutions designed to generate knowledge and/or to translate knowledge into products. I rely here on the economist Richard Nelson's use of the term "institutions" as referring to "structures that define or mold the way economic agents interact to get things done"—"such as universities … ; government agencies … ; banks … ; organizations and laws … ; widely shared beliefs and values and customs."[26] For Nelson, institutions are "social technologies" distinct from but co-evolving with "physical technologies" and industry structure. Indeed, we will see that the physical and social technologies for microelectronics are so tightly coupled that it can be hard to cleanly distinguish the two.

Still, the microelectronics industry's innovations in physical technologies are better known than its innovations in institutions. In fact, even the pace of innovation in physical microelectronics technologies has become famous as "Moore's Law," the rule of thumb that gives this book its name. There are several formulations of "Moore's Law," but roughly it says that the density of circuit components per unit area that it is most economically profitable for commercial, high-volume manufacturers to cram onto silicon doubles over a constant time period—about a year in 1965, when Gordon Moore first articulated his observation, but closer to two years since the mid 1970s.[27]

Clearly, that definition emphasizes innovation in Nelson's "physical technologies." The number of transistors, resistors, and inductors that fit in a square centimeter of silicon is a physical measure. Note, though, that Moore's observation was also about economics—what density would be most profitable—and therefore it was unavoidably related to institutions such as the business models employed by semiconductor firms. Across the board, the innovations in physical technologies that have kept Moore's Law accurate have co-evolved with innovation in institutions.

For instance, Moore's Law has been aided partly by occasional leaps in the standard size of the silicon wafers used in semiconductor factories ("fabs").[28] That may not sound impressive, but changing standard wafer sizes requires complex coordination among manufacturers, materials suppliers, and equipment vendors—many of whom compete viciously when not cooperating on matters such as wafer size. Another front in increasing chip density is better "architectures" for puzzling together components in a chip. Chip architecture, especially computer assisted design of such architectures, has long been a locus of academic research at universities such as Caltech and Berkeley. We'll see that significant institutional innovations were necessary in order to allow competing firms to cooperate on issues such as wafer size, and in order for universities to collaborate with firms on topics such as chip architecture.

The most astonishing physical aspect of Moore's Law has been the exponential decrease in size of electronic components. In 1947, when the transistor, the essential component of conventional solid-state circuits, was invented, it was assembled by hand from macro-scale parts, including a paper clip. Today, transistors in commercial chips contain features only a few atoms wide—too small for a human to see, much less assemble.[29] Instead, transistors are formed from raw silicon in a complicated, largely automated process involving dozens of steps, each utilizing an array of multi-million-dollar tools incorporating decades of research, development, and industrial coordination, all taking place in the cleanest rooms on Earth.

Once again, innovation in the physical technologies of miniaturization has been inseparable from innovation in institutions. For example, machines and people are so tightly choreographed in a clean room that modern semiconductor manufacturing would be impossible without innovations in labor discipline.[30] At a much larger institutional scale, the physical technologies of semiconductor manufacturing have also co-evolved with the structure of the entire microelectronics industry. That's because, as they have evolved, the tools, materials, and clean rooms needed to mass-produce chips containing a billion transistors have become astronomically expensive. The increasing cost of semiconductor manufacturing means a decreasing number of entities can engage in it. As *Science* put it in 1980, "It now takes a big company to make miniaturized microcircuits, and this new development is changing the nature of the industry."[31] Almost thirty years later, the innovation scholar Clayton Christensen was saying the same thing:

> To amortize the $5 billion investment in a fab over a five-year schedule costs more than $3 million a day. … Moore's Law has been responsible for the[se] gigantic costs. … Smaller companies (and even some sovereign states) can no longer afford to play the game. A massive restructuring in the industry is forcing them to consolidate or outsource production.[32]

The increase in the cost of the equipment needed to manufacture smaller transistors has given rise to new types of organizations such as "foundries" that make chips to other firms' designs—hence Christensen's reference to "outsourcing" production. In fact, today's semiconductor manufacturers outsource almost everything in order to stay lean and agile enough to keep pace with Moore's Law. That includes outsourcing research, especially basic or long-range research. One finding of this book is that the novel organizations that formed since the late 1970s to enable the semiconductor industry to outsource research—industrial consortia and university-industry research centers—have provided templates for the reorganization of American science in general.

Despite its cost, miniaturization benefits makers and users of electronic equipment in many ways. In fact, certain benefits of miniaturizing circuit components were well known several decades before the formulation of Moore's Law. For instance, compact vacuum tubes and batteries (for hearing aids, infantry radios, proximity fuzes, and surveillance devices) date to the 1930s and the 1940s.[33] Miniaturization made such devices more portable, durable, faster, and easier to hide or disguise. In the 1950s, tubes gradually gave way to solid-state transistors, and in the 1960s individual transistors were slowly displaced by transistors embedded in integrated circuits.[34] Crucially, miniaturizing integrated circuit components allowed the price per transistor to drop, since as transistors got smaller the same number of costly steps needed to process a particular area of silicon would yield more transistors within that area. This, roughly, was the argument of Gordon Moore's 1965 paper. However, Moore observed that, beyond some level of miniaturization, extant manufacturing processes would yield an unprofitably large number of defective transistors within that constant area. For any particular manufacturing process, there would be an optimal number of transistors within an area—but as manufacturing processes improved, that optimal number would increase.

Thus, Moore's Law meant that every year manufacturers could make more transistors per chip at lower unit cost. Lower cost led to greater demand which in turn meant even greater economies of scale. In 1947 there was one transistor in the world; twenty years later, several billion transistors were sold per year for about a dollar each.[35] Today, a single chip containing more than a billion transistors costs a few hundred dollars. Even before the fortieth anniversary of Moore's Law (2005), the semiconductor industry was making more transistors per year than the world's farmers were growing grains of rice—and at lower unit cost by two orders of magnitude![36]

Miniaturization, then, facilitates production of cheaper integrated circuits. It can also improve circuit speed—in general, electrons transit a circuit more quickly as the circuit gets smaller. The weight of the circuit also decreases. Conversely, miniaturization means that the complexity of a circuit that can be built at a constant weight and price goes up every year. Miniaturization, therefore, was critical to making certain long-imagined applications feasible. For instance, it didn't become possible to package a telephone, a camera, and a computer into a single hand-held device until miniaturization reached a certain level, and the same miniaturization allowed the cost of such devices to be reduced so much that mobile smartphones are now widespread in industrialized and industrializing countries alike.

Semiconductor manufacturers strive to make smaller transistors in order to sell more, cheaper, faster, more versatile chips. But why should the pace of miniaturization be regular enough, over many decades, to be described by Moore's Law? The answer, in part, is that some hand-waving is necessary to fit the data to a law-like curve. Gordon Moore himself acknowledges that the doubling period changed from 12 months to 24 months between 1965 (when he was director of research at Fairchild Semiconductor) and 1975 (the year he became president of Intel, the start-up he co-founded with Robert Noyce in 1968). Historians and management scholars, particularly Ethan Mollick, have shown that there have been other modifications to Moore's Law, each adjusting the metric to fit the data.[37]

Thus, Moore's law is not a law of nature in the way that $E = mc^2$ is. Most historians of the semiconductor industry believe, instead, that Moore's Law has become a "performative device" that guides rather than predicts action; as the sociologist Donald MacKenzie says of financial models, Moore's Law is "an engine, not a camera."[38] Such an engine is needed because manufacturing semiconductors is both expensive and complex. As was noted above, expense undermines vertical integration; few companies can afford to master more than a fraction of the process of manufacturing wafers. Thus, many different companies contribute to making a chip. However, the complexity of semiconductor manufacturing means that those companies have to work together closely. Even trivial changes to one process step can have ramifications for many other steps. Vendors specializing in that step must therefore coordinate with those involved in other steps. Moore's Law is a tool for that coordination. By setting a pace for miniaturization, Moore's Law tells everyone involved in semiconductor manufacturing what benchmarks they will have to meet at the same time as their collaborators and slightly ahead of their competitors. Industry-wide planning mechanisms such as the International Technology Roadmap for Semiconductors enshrine Moore's Law in a vast array of targets covering every conceivable aspect of microelectronics manufacturing. Vendors that miss those targets are penalized by manufacturers, and manufacturers that miss their targets are penalized by consumers.

But why coordinate around a doubling of density every two years rather than some shorter or longer period? Some observers—among them the futurist and former *Wired* editor Kevin Kelly—argue that the pace of miniaturization is governed by microelectronics technology itself, rather than by the conscious choices of human agents.[39] All humans can do is match the pace of innovation allowed by the underlying physics. To historians and sociologists of technology, Kelly's view sounds like technological determinism—the idea that technology makes us, and itself, more than we make it. Indeed, one historian, Paul Ceruzzi, has provocatively argued that Moore's Law is better evidence for technological determinism than historians can offer against it.[40]

My own view leans toward the performative-device perspective, but this book will be broadly compatible with the Kellian technological-determinist explanation. Instead of choosing one or the other, I approach Moore's Law as a "social fact" that is continually made into empirical fact by actors' perception of Moore's Law as empirical fact. Social facts are the subject of a vast literature by social theorists and philosophers such as Anthony Giddens and John Searle, but they are also recognizable in everyday experience.[41]

Money and marriage, for instance, are social facts much as Moore's Law is. An array of institutions, mores, practices, and perceptions continually reinforce prevailing definitions of money and marriage so that any individual can do little to change those definitions. Such social facts rest on agreed-upon, somewhat arbitrary conventions. Most bureaucracies, by conventions to which they have agreed, are persuaded that I am married by a piece of paper that bears a judge's signature, and most shops have agreed by convention that I can trade other pieces of paper in my wallet for their goods and services. These conventions can be quite stable so long as most actors, most of the time, take the conventions as given. And yet, conventions can come undone, sometimes quickly and dramatically. No law of nature or logic prohibits an individual from inventing his or her own currency or declaring himself or herself married to (or divorced from) anyone or anything, but one will face great difficulty in persuading others to turn those actions into durable social facts—just ask Henry VIII! Yet currencies come and go, and the nature of marriage changes. Social facts are both malleable and durable.

Similarly, few individuals can do much to make Moore's Law go any faster or slower. For most of us it is a given, upon which we rely in making decisions. As the computer in front of me obsolesces, Moore's Law helps me decide when to replace it. As an individual, I react to the pace of miniaturization much more than I shape it. But if large numbers of individual consumers were to change their preferences, Moore's Law would change. For example, if mobile phone users were to decide that they had all the speed and memory they wanted but would like phones to consume less power, the pace of miniaturization might slow as research and resources shifted from the miniaturization problem to the power problem.[42]

Social facts are not only social, of course. For many people, marriage is what it is because of what they believe to be transcendent laws of the deity or biological laws of human behavior, and for much of recorded history money has been defined in part by the physical scarcity of gold. Similarly, Moore's Law is connected to the physical laws governing silicon and other materials. Indeed, this book would be pointless if it weren't. Most of the people mentioned in the following chapters hoped to accelerate the discovery of physical laws that would keep miniaturization going. Just as *how* religion and biology ought to shape definitions of marriage and *how* the availability of gold ought to shape definitions of currency are open to debate and change, *how* physical law informs Moore's Law is open to debate, to contestation, and to human, social action. Different organizations and communities of researchers have understood physical reality as opening up different paths toward miniaturization. The laws of nature—as different social groups understood them—informed those views. However, those communities' and organizations' views on how microelectronics should progress were also informed by more obviously human forces, such as economic competition, patriotism, desire for professional prestige and scientific discovery, fascination with gadgetry, and organizational politicking.

Innovation and Conditional Prophecy

Like any social fact, Moore's Law is durable—if everyone agrees that it is durable. For more than fifty years, the miniaturization of silicon circuits has been a stable fact of life. If, like Kevin Kelly, Ray Kurzweil, and other techno-visionaries, you count pre-integrated circuit devices (electromechanical switches, vacuum tubes, early discrete transistors), then something like Moore's Law has roughly held for more than 100 years.[43] That longevity is due in part to the law's malleability and conventionality. The metrics by which Moore's Law is measured change occasionally, but by convention those changes have maintained, rather than undermined, the law's accuracy. But Moore's Law also endures because it presents a reality that is objective enough so that most participants rely on it as an horizon for action, and in doing so endow it with objective reality. Because it has endured so long, people reasonably assume that it will continue to hold true for a good while longer, ten years being an oft-quoted horizon.[44] People therefore assume that their competitors and their collaborators will continue to keep pace with Moore's Law, because those competitors and collaborators assume that their competitors and collaborators will do the same. Everyone takes steps that maintain Moore's Law, even if most actors believe that they are simply keeping up with Moore's Law.

Yet, like many social facts, Moore's Law is as fragile as it is durable. Even before Gordon Moore quantified the miniaturization trend, some researchers and policy makers were declaring that trend nearly over.[45] Moore's Law has always already been in crisis. In that respect, silicon is like oil. As long ago as the nineteenth century, some informed observers were saying that the world's oil would soon run out.[46] It is, of course, reasonable to believe that the supply of oil is finite. Yet, oil continues to flow, because more capital-intensive and technology-intensive means of finding new oil become profitable as easy oil becomes scarcer. Similarly, Moore's Law should end someday. Computers consist of logic elements that cannot be made any smaller than a certain size. There is a long tradition (examined in later chapters) of calculating the "ultimate limits" to the size of transistors and predicting the demise of Moore's Law's on the basis of those calculations. Yet as more capital-intensive and technology-intensive means of making smaller circuits have become profitable, transistors have repeatedly gotten smaller than the most credible calculations of ultimate limits had predicted.

As I write, in the summer of 2016, declarations that Moore's Law is over have become very loud.[47] Those declarations may be right, though inductive logic would lead one to disregard them as instances of unfounded alarmism (as their predecessors were). I propose a third alternative, however: that declarations of Moore's Law's end are not statements with binary truth value, but rather are conditional prophecies which bring about changes that prevent the prophecy from coming true. The conditional prophets of the Old Testament and other religious traditions warned their listeners that God would smite them for their wickedness, yet often those warnings caused listeners to return to virtue and postpone God's wrath. Similarly, the conditional prophets of microelectronics have stimulated a variety of experiments and reforms that have kept the semiconductor industry's doomsday from coming to pass. New research fields and new institutions have resulted from these conditional prophecies.

Moore's Law is susceptible to conditional prophecy because the microelectronics industry is like Lewis Carroll's Red Queen: To stay in the same place, firms run faster each year. New technical innovations are needed every year to continue increasing the density of chips.[48] However, the complex interactions among different steps in the process of manufacturing integrated circuits require that any changes to the manufacturing process be planned far in advance. The conditional prophets of Moore's Law help focus that planning onto particular technical factors that must be dealt with if the prophet is to be proved wrong. One recurring theme of this book, though, is that the technical predictions of Moore's Law's prophets are often entangled with their efforts at community building and institutional innovation. By assembling and guiding networks of researchers, these prophets help create the conditions under which the obstacles to advancing Moore's Law can be overcome. In the chapters that follow, we'll see numerous examples in which institutional innovations linking boardrooms, factory floors, federal agencies, and academic laboratories were justified by conditional prophecies of the end of Moore's Law.

Institutional innovation and heterogeneous network building have become increasingly necessary to extend Moore's Law because the past fifty years have seen a decline in microelectronics manufacturers' capacity and willingness to do basic, rather than applied, research. The distinction between basic and applied is not clear-cut, of course, and I will examine several individuals and organizations that did both. Still, research managers such as Gordon Moore routinely differentiate between basic and applied, in part by differences in how the quality of research is measured. Basic researchers get praise when they make fundamental discoveries. Society sometimes assigns such discoveries intrinsic worth—for example, many members of the public are enthusiastic about reports of new dinosaur finds or of observations of new high-energy particles even if they can't make practical use of the discoveries. At other times, society assigns value to fundamental discoveries on the basis of their serendipitous or long-term applications. Applied researchers get praise when, over the short term, they create intellectual property and solve specific technological problems their patrons have assigned them.

The two semiconductor-manufacturing firms Gordon Moore co-founded, Fairchild Semiconductor and Intel, were famous for (at least initially) minimizing in-house basic research as compared to larger, established firms, such as IBM and RCA.[49] At Fairchild that strategy had deteriorated by the mid 1960s, and its deterioration contributed to Moore's decision to leave Fairchild and co-found Intel. As he put it in 1994,

> I had gotten very frustrated at Fairchild running the laboratory. As the manufacturing group got technically more competent they were less willing to listen to the people in the laboratory as the experts. So, when we came up with some new idea, for example stable MOS devices, we had great difficulty transferring the detailed instruction of how to do that to manufacturing. We were much more effective in transferring it to the spin-off companies than we were to the organization internally. And, we decided to avoid that at Intel, in that we didn't set up a separate laboratory. We said "Well, heck, we'll do the development in the manufacturing facility."[50]

Intel's success under Moore's leadership taught other firms that they could compete better if they focused their in-house efforts on applied research and left basic research to others. Moore's philosophy of corporate research spread quickly in the 1970s.

Few research managers, however, believed that the US semiconductor industry could compete if no one was doing basic research. There would be no Moore's Law without inputs of personnel trained in basic research, or without fundamental insights and accidental discoveries made in the course of basic research. To benefit from basic research while minimizing the burden of conducting it themselves, microelectronics firms increasingly shifted responsibility for basic research onto universities, consortia, national laboratories, and other organizations. They also increasingly shared the costs of supporting basic research among one another, with state governments, with the federal government, and with philanthropic foundations. As a result, those involved in (or wanting to become involved in) the semiconductor industry have energetically experimented with novel mechanisms for bringing together university, industry, and government stakeholders. Indeed, the alliances necessitated by the semiconductor industry's outsourcing of research offer a notable example of Henry Etzkowitz's conception of science as a "triple helix" interlinking government, academia, and industry.[51] As Thomas Misa and Stuart Leslie have shown, firms, universities, and government agencies were already working closely together on microelectronics research in the early Cold War.[52] But changes in the finances, the aims, and the practices of American microelectronics firms, universities, and state agencies from the late 1960s on contributed to a "de-naturing" of the postwar "triple helix" and then to numerous attempts to re-link the strands in new ways that would ensure the survival of the domestic semiconductor industry.

Perspective and Evidence

This book should be read largely as a history of the continual experimentation within and among government, academic, and industrial institutions that has accompanied the more visible technological innovation underpinning Moore's Law. I adopt Charles Sabel and Jonathan Zeitlin's "historical alternatives" approach to understanding how organizations simultaneously experiment with technologies and institutions. Sabel and Zeitlin's starting premise is that "multiple efficient combinations of capital equipment, factor supplies, and human resources are typically possible."[53] Organizations therefore continually try new configurations of technology and organizational structure. Across a set of comparable organizations (say, the firms in an industry or the departments in an academic discipline), shifts from one "best" configuration to another are rarely sudden or complete. Instead, there may be a gradual tipping away from one predominant configuration and toward another as actors weigh new alternatives against old ones.

The changes in American science that occurred late in the twentieth century were not a rupture or an epochal break; rather, there was a proliferation of experimentation with, and a gradual transition to, various alternatives to the early Cold War paradigm. Like laboratory experiments, though, institutional experiments rarely work on the first try. Many institutional experiments brought some (often unforeseen) benefits to their stakeholders, but still fell short and had to be amended. Even the experimental university-industry-government partnerships that endured periodically faced externally and internally imposed changes in their parameters and objectives. The problems that an institution is tasked with solving today are often not the problems it was originally supposed to solve. In particular, many of America's most important research institutions were originally designed to aid in defeating communism and winning the Cold War. Those institutions are still vibrant today because they took up a new list of problems even before the Cold War ended—a list that prominently featured the need to aid US semiconductor firms' global competitiveness.

In examining how the semiconductor industry ended up on that list, I will not, for the most part, look at the semiconductor industry itself. Instead, I will examine the long shadow cast by the semiconductor industry onto American universities and government agencies.[54] The organizations most central to maintaining Moore's Law—Silicon Valley firms and their competitors and collaborators on the East Coast of the United States and in Asia and Europe—do not play leading roles in the narrative. Instead, the following chapters look at universities, government laboratories and funding agencies, academic journals and conference series, philanthropic foundations, and professorial start-up companies. None of these entities manufactured commercial circuits themselves. Therefore, all of them had to innovate, both in physical and social technologies, in order for the semiconductor industry to take them seriously. Some big, established semiconductor firms (notably IBM) do appear in several chapters, but I focus on those firms' eccentric, struggling "side bets" rather than on their mainstream approaches to microelectronics innovation.

It is possible to look directly at the semiconductor industry, and other scholars have done so better than I could.[55] In some ways, though, we inhabit Plato's cave and can access the shadows but not the objects casting them. For instance, information about microelectronics research is easier to obtain from the sideshow of the semiconductor industry than from the main event. Government agencies and universities are generally more careful about preserving their ephemera and making them accessible than corporations are, so much of the evidence presented in this book comes from federal and university archives. Government and university microelectronics researchers generally have fewer restrictions on dissemination of information than their corporate colleagues, so much of what you will read comes from interviews with government and academic scientists and engineers. Some interviewees—again, usually those in government or academia—also shared important documents with me.

The danger in using these sources is that they provide a skewed view of which actors were important. It certainly isn't my intention to exaggerate the importance of universities, government agencies, or other marginal actors in the development of microelectronics and computing. However, there is much we can learn about the center of semiconductor manufacturing by looking at how actors at that center explained their partnerships with those at the periphery (and how those at the periphery explained their partnerships with those at the center). That said, this book is less about the evolution of commercial microelectronics than about the experiments in funding, directing, and conducting research that were intentionally or unintentionally influenced by the semiconductor industry's "long arm." In doing so, my objective is primarily to provide a new explanation (complementary to previous ones) for the specific historical question of why American science has changed since 1965. Secondarily, I aim to cast light on the generalizable phenomenon of a "long arm" by which one thicket of institutions (e.g., an industry, a religion, or a state) can rearrange and/or bring closer another thicket of institutions, especially those involved in the creation of knowledge. These objectives take priority over, though they don't preclude, any contribution I make to studies of semiconductor manufacturing *per se*.

To understand the long arm of Moore's Law, then, we need to look at a thicket of institutions far removed from semiconductor fabs. Moore's Law affected not just people and institutions who were already contributing to semiconductor innovation, but also those who were not contributing to innovation in semiconductor manufacturing but wished to, or were not reaping the benefits of semiconductor manufacturing innovation but wished to, or had been contributing to innovation in semiconductor manufacturing but were pushed into other domains by the creative destruction associated with Moore's Law. The places where American science is funded, conducted, and steered appear prominently in those categories. In the decades since Gordon Moore proposed his law, government laboratories and research funding agencies, universities, corporate laboratories, and high-tech start-up firms all adopted innovations with the aim of helping the US semiconductor industry outpace global competitors and keep pace with Moore's prediction. Moreover, those institutional innovations spilled over to affect how American science was done in fields seemingly far removed from electrical engineering and semiconductor physics.

The semiconductor industry's long arm is not unique. In particular, the Cold War national-security state possessed an even longer reach, reshaping work in almost every field of the natural, social, and engineering sciences in universities, federal labs and agencies, philanthropic foundations, and corporate research labs.[56] Indeed, the semiconductor industry only emerged when the long arm of the military reached into a thicket of universities, start-up companies, and regulated-monopoly firms to assemble a new constellation of institutions that could provide it with high-speed circuits. From the late 1960s on, however, civilian markets quickly became more important than military markets to the semiconductor industry. Semiconductor firms' arms extended into far-flung institutions of American science just at the moment when the long arm of the national-security state retreated.

The national-security state hasn't disappeared; it still exerts plenty of influence over American science. Nor have the changes fostered by the semiconductor industry diffused across all of American science—in most disciplines, for instance, faculty members who patent their work and start companies are still exceptions. One reason I draw on Sabel and Zeitlin's "historical alternatives" approach is that we are still in the midst of exploring alternative ways of organizing science. That means it is possible to examine the historical actors' behavior without knowing how the story ends, and without knowing what the "right" choices would have been. I haven't always avoided teleology, but in general I have tried to show why various actors thought different institutional experiments were reasonable or unreasonable at the time, rather than with the benefit of hindsight.

Moreover, because most of the actors in this story are still alive, I have been able to talk with them about that process of exploring alternatives. Interviews are of course problematic, since interviewees recount the past from a position in the present. But interviews offer evidence about historical alternatives not usually found in print—for instance, about interviewees' affective attachments to particular alternatives, or the role of charismatic personalities in pushing institutions toward one alternative or another. In chapter 6, I even offer insights gleaned from working alongside a few actors—rather than just interviewing them—as they explored different ways of making American science more responsive and responsible to a wider variety of stakeholders.

But which actors? My argument is that the changing structure, markets, and technologies of the US semiconductor industry gave rise to a variety of institutional experiments, some of which have propagated well beyond fields with obvious connections to microelectronics. I therefore focus on the types of actors with the most capacity to propose, enact, oversee, and judge among institutional experiments. These actors populate what Thomas Misa has called a "meso-scale" approach to the history of technology, as opposed to a micro-scale examination of machines and artifacts or a macro-scale examination of broad cultural trends.[57] On an organizational chart, an actor at the meso scale is, as the name implies, in the middle—for example, not a university president or a struggling graduate student, but perhaps a director of a research group or interdisciplinary academic center or university technology transfer office; not the president of the United States or a principal investigator on a single federal grant, but perhaps a federal program officer overseeing a portfolio of grants or an executive in a federal agency overseeing a handful of program officers.

My focus on meso-scale actors is not meant to exaggerate their contributions or to minimize those of others. Rather, meso-scale actors offer a convenient view of what kinds of institutional innovations were thought possible, were tried, and were discarded, amended, or kept in response to the needs of the semiconductor industry. It is by talking to and reading about meso-scale actors that we can get some sense of how the semiconductor industry's long arm exerted influence over scientific knowledge-making. Unfortunately, focusing on meso-scale actors largely minimizes attention to those who almost always get minimized in American science: women; people of color; people with disabilities; people who identify as lesbian, gay, bisexual, or transgender; the working class; political radicals; immigrants. Indeed, focusing on the meso scale offers ways to interrogate the processes of exclusion. Under-represented groups are under-represented in science not just in the laboratory, but also—maybe especially—in the kinds of positions where institutional innovations are crafted and judged.

Exclusion from the meso scale can, therefore, be self-reinforcing. Often enough, institutional innovations are crafted by those with privilege. We'll see, though, that the meso-scale actors in this book frequently came in contact with members of various groups that are under-represented in American science, including campus radicals, sight-impaired men and women, and students and faculty members at historically black universities. Some of the institutional innovations we'll examine were meant to stymie these groups, but in some cases members of these groups contributed to institutional innovations by which they could have a greater say in American science. The political and cultural environment in which American science takes place was changing—particularly around 1970 and again in the 1990s—in ways that occasionally made the meso stratum larger, more diverse, and more democratic. The semiconductor industry participated in that story, in part because of the need to work with new kinds of users as microelectronics civilianized and in part by providing a model for how to adapt the national-security research infrastructure to solve new kinds of civilian problems as the Cold War gradually and then suddenly eased. In extending their reach into American science, semiconductor firms—mostly without meaning to—simultaneously extended their reach into the heart of American democracy.

Motivation of Chapters and Concepts

The discussion thus far has been abstract. In preparation for what will follow, let's examine a concrete episode that marks the turning point of the book's narrative: the opening of a new, industry-funded building for the Center for Integrated Systems at Stanford University, one of the premier sites for university-industry collaboration in microelectronics.

In May of 1985, the director of the Center for Integrated Systems, James Meindl, wrote to Erich Bloch, the director of the National Science Foundation, to explain the CIS and invite Bloch to speak at its opening:

> [T]he CIS is to combine two technological cultures—integrated circuits and information systems—in a manner that produces a whole—integrated systems—that is greater than the sum of the parts. … A second key feature of the CIS might be called a sociological innovation because it blends two sociological communities—industry and academia—in a manner that again aspires to produce a whole greater than the sum of the parts. … Finally, the importance of university-industry-government cooperation in new ventures such as the CIS would indeed be a stimulating topic for this occasion.[58]

Here we see, in correspondence between two exemplary meso-scale engineers, the recurring themes of this book: "sociological [or institutional] innovation," interdisciplinary collaboration, and university-industry-government cooperation.

The opening of the CIS' new building is the fulcrum of my narrative because before then interdisciplinary academic microelectronics centers funded jointly by industry and government were still highly experimental and somewhat rare. The first half of the book looks at such centers and also at other institutional and technological innovations that proliferated at the margins of the semiconductor industry in the 1970s. The speech that Erich Bloch eventually gave at the opening of the CIS perfectly articulates why he and other meso-scale actors believed they needed institutional experimentation in the 1970s, and why some of those experiments (but not others) were coming to fruition by 1985:

> In the 1970s the federal government changed course. There was a decline of emphasis on basic research. Funding was reduced, especially for institution-building programs. Funding for specific applied research was increased. The idea of a 'quick fix' through research predominated, including in such areas as energy, housing, cancer research and crime. Industry's focus on basic research also slipped in the 1970s. … The university research effort suffered, and the pool of talented scientists and engineers teaching and working in academia stagnated. The poor state of research instrumentation in many university laboratories is another heritage of that time. But things have changed in the 1980s, and our policies are changing as well. The current period is characterized especially by a vigorous economic challenge from other countries.[59]

Here we see the breakdown of the early Cold War "triple helix" and the "failed" (at least to Bloch) search for a more socially relevant way of doing science in the 1970s.

We also see Bloch gesture to global economic competitiveness as the new overriding rationale for public investment in science. That rationale underwrote changes in American science that were designed to aid the semiconductor industry more than any other. The American steel and auto industries also struggled to compete, especially with Japanese firms, in the 1980s. But more than those industries, semiconductor firms required long-range research, which government could help supply by supporting institutional innovations such as industrial research consortia and academic microelectronics centers such as the CIS. In the first half of this book, we see how the rationale of global economic competitiveness jockeyed with other rationales—such as alleviating racial injustice or environmental degradation—as a defining objective for American science. By 1985, economic competitiveness had become the favored rationale of policy makers, and other rationales were appended and subordinated to it. As a result, centers such as the CIS went from fragile experiments to much-copied models. In the second half of the book, we look at a world in which academic microelectronics centers had become stable and common, even as the skyrocketing manufacturing costs associated with Moore's Law forced those centers to continually revise their relationship with the semiconductor industry.

I develop that narrative through a series of case studies, each of which puts forward a different aspect of Moore's Law's long reach. Chapter 1, for instance, examines the role of novel research on integrated circuits in revising the aims and the patronage of American academic research in the late 1960s. This was Bloch's era of the "'quick fix' through research," when academic scientists and engineers were expected to apply their expertise to civilian social issues (health care, the environment, disabilities, poverty, race relations, and so on). To see how microelectronics became attached to those topics, I follow James Meindl and his colleagues at Stanford as they navigated the student protests of the late 1960s and the early 1970s. Stanford might seem too obvious a choice for a case study, in view of how much scholarly and popular attention its ties to Silicon Valley have received. However, Stanford's electrical engineers really were at the forefront in tying a technological innovation (low-power integrated circuits) together with a variety of institutional innovations designed to overcome the challenges of the years around 1970.

Low-power integrated circuits similar to Meindl's eventually became the predominant form of commercial integrated circuits, thanks largely to Gordon Moore and Intel. IBM, however, stuck with more traditional bipolar transistors, not converting until the early 1990s, when the firm almost went bankrupt. Today, historians view IBM as a distant follower in the move away from bipolar transistors. In the 1970s and the early 1980s, though, IBM was the leader of its industry, especially in manufacturing integrated circuits for computing. After all, the mainframe computer industry was known as "IBM and the Seven Dwarves," and in personal computing the generic term "PC" meant an IBM product or a clone of one. The 1970s and the early 1980s also saw dramatic gains in the stature and influence of IBM's research arm. Any account of how consensus was forged regarding which kind of circuits would best advance Moore's Law should, therefore, consider IBM's favored strategy—even if that turned out to be the losing strategy.

I show that IBM researchers and executives didn't cling to their strategy blindly. They knew that bipolar transistors might not remain profitable, and they invested enormous sums in exploring exotic alternatives to bipolar transistors, two of which are the subjects of chapters 2 and 3. Chapter 3 surveys IBM's attempt, running from the mid 1960s to the early 1980s and costing more than $100 million, to switch from semiconductor circuits to circuits based on superconductors (materials that lose all resistance to the flow of electrons at low temperatures). Chapter 3 follows a young IBM chemist named Ari Aviram who dreamed of something even more radical than superconducting computing: a computer made from single-molecule logic elements. Aviram couldn't take his ideas very far within IBM; however, those ideas soon migrated to the Naval Research Lab, where a charismatic institutional entrepreneur named Forrest Carter began forging a global research community. Carter attracted publicity and inspired pioneering research with his vision of molecular computing, but he also contributed to the field's lasting reputation for grand promises, vague details, and unrigorous experimentation.

There were other exotic alternatives to silicon, both inside and outside IBM, that I could equally have chosen in order to examine the long arm of Moore's Law. However, the combination of IBM, superconducting computing, and molecular electronics allows me to develop two concepts over the course of chapters 2 and 3: the "zig-zag" model of innovation and the "network failure" model of innovation policy (the latter borrowed from Josh Whitford and Andrew Schrank).[60] The zig-zag model is my amendment to the famous "linear model" according to which technologies reach society at the end of a linear process in which discoveries move from fundamental research to specific application. The two IBM cases show, however, that in practice researchers switch the arrow of progress back and forth, opportunistically going from applied research to basic science as often as they move from basic to applied. By offering sufficient resources, patrons—especially the state—can of course encourage researchers to move linearly from basic to applied research. But chapters 2 and 3 show that by the mid 1970s the American national-security state was losing its ability to encourage clients to adhere to the linear model. Forrest Carter and other representatives of the federal government turned to hosting mixed communities of corporate, academic, and government researchers in hopes that the networks linked together by those communities might bring much-needed technologies to fruition at less cost to the government.

Carter's molecular electronics network faltered with his death in 1987, in part because that network was held together by Carter's charisma rather than by an institution that could outlive him. Chapters 4–6 examine an institutional innovation that did prove capable of durably hosting mixed networks of researchers at the margins of the semiconductor industry: the academic microfabrication facility. These facilities arose in the mid 1970s from worries that the ability of academic electrical engineers to aid industry was being eroded by the factors Erich Bloch cited in the passage quoted above: waning support for basic research and the "poor state of research instrumentation in many university laboratories." Chapter 4 looks at an early organization intended to alleviate that problem—the National Research and Resource Facility for Submicron Structures at Cornell University—and the "organizational field" (a term borrowed from organizational sociology) that arose in the 1980s and was composed of other academic microfabrication centers that copied and/or attempted to supersede the NRRFSS model.[61]

Academic microfabrication centers proliferated at the same time as a series of semiconductor research and development consortia that served the microelectronics industry. Indeed, chapter 5 makes the case that these centers should be seen as—or at least as adjuncts to—industrial consortia. Stanford's Center for Integrated Systems and Cornell's NRRFSS shared many characteristics with and ties to industrial semiconductor research consortia. In the early 1990s, however, the National Science Foundation pushed the Cornell and Stanford centers together as co-leaders of an academic consortium, the National Nanofabrication Users Network (NNUN). The NNUN bridged universities with industry and the engineering sciences with the life sciences in a "network of networks"—an overlapping topography of connections among different kinds of constituents. With the end of the Cold War and, even earlier, the easing of the national-security state's dominance of semiconductor markets, building networks of networks became an attractive means for the state to meet multiple objectives at once: supporting the domestic semiconductor industry, ameliorating racial inequalities in science, extending Americans' lifespans, and so on.

As the name of the NNUN implies, academic microfabrication centers began to change their names in the late 1980s and through the 1990s to names containing the prefix "nano." In fact, I will show throughout the book that several prominent figures in extending the long arm of Moore's Law were also influential in the field that came to be known as nanotechnology. Nanotechnology is many different things, but in part it is a tool for taking institutional innovations developed to aid the semiconductor industry and extending them across the scientific landscape. At the same time, nanotechnology generated connections and institutions that allowed some researchers to move away from dependence on the semiconductor industry. Chapter 6 develops that point by looking at the interwoven development of the molecular electronics community and the organizational field of academic microelectronics research centers during the 1990s and the early 2000s. In that chapter the "long arm" concept comes to the fore as I examine how a grand vision associated with microelectronics—a molecular computer—provided a justification for restructuring individual and collective research programs across a wide swath of American science.

Throughout this book, readers will meet many such actors and entities who aimed to reconstitute the American scientific enterprise in the light of the semiconductor industry's needs and/or capacities. Over the arc of the book's chapters, the overlapping, interlocking interactions of these people and organizations will, I hope, gradually become apparent. Along the way, I will try to show how these recurring characters became vectors for many of the most notable macro-scale changes in American science since 1970. In an epilogue, I summarize some of those changes as a shift in emphasis of American science toward centers and consortia, networks and networks of networks, the life sciences, and civilian markets.

The microelectronics industry was not alone in the ongoing process of changing American science, but since the mid 1960s it has been omnipresent and vigorous in that process. The epilogue closes by arguing that any reckoning with what has happened to science in the United States and elsewhere in the past fifty years is doomed if it neglects the intended and unintended consequences of that industry's clockwork tempo of innovation and appetite for research. Moore's Law so pervasively paces change in the institutions of science that its influence sometimes disappears from view. But when we lose sight of it, we let slip one of the keys to a more responsive and responsible kind of science.

1 Crisis and Opportunity at Vietnam-Era Stanford

In 1967, James Meindl left his job as a senior researcher and grant officer in the Army Signal Corps' lab at Fort Monmouth in New Jersey and moved to California with his young family to join Stanford University's Department of Electrical Engineering. Meindl's job change exemplified many of the transitions that were just beginning to affect American microelectronics: from government (especially military) customers to private, civilian markets; from the East Coast to the West; from universities as producers of personnel to universities as producers of innovations and intellectual property; from the bipolar junction transistor to the metal-oxide-semiconductor field-effect transistor (MOSFET). I'll explain the differences between these transistor types later. For now, it will suffice to say that MOSFETs later became the dominant commercial transistor type, but that in the late 1960s they were one of the less likely of several alternatives to bipolar transistors being explored in firms and at universities.

This chapter will show that the exploration of MOSFETs as a technological alternative co-evolved with the exploration of various institutional experiments: academic facilities for fabricating integrated circuits, faculty patenting and entrepreneurship, partnerships between universities and extramural stakeholders such as firms and secondary schools. In the short term, Meindl's institutional and technological experiments with MOSFETs helped Stanford deal with shortfalls in funding and in political legitimacy that constituted the worst crisis in the university's history. Over the longer term, some of Meindl's experiments became less experimental—that is, they became part of the new way of doing science on American campuses. Along the way, though, their origins in the university's Vietnam-era turmoil largely disappeared from view.

Stanford's turmoil of the late 1960s had many roots. But one reason for the university's troubles that most observers agreed on—even as they disagreed on everything else—was that the university had become too dependent on support from the national-security state. In the late 1940s, Stanford had been one of the first and savviest universities in adapting to the postwar research funding paradigm dominated by the military and allied agencies, such as the Atomic Energy Commission. By 1967, though, that regime was under siege, and even its proponents agreed that Stanford should diversify its funding.

Stanford found itself in that position because in the early postwar years Frederick Terman—first as dean of engineering and then as provost—steered the university to take advantage of new winds from Washington.[1] Doing so required an energetic campaign to transform a sleepy provincial college focusing primarily on undergraduate education into a powerhouse of high-tech innovation. Above all, Terman's plan emphasized research and PhD education over undergraduate teaching as the quickest way to climb in the academic rankings and to secure federal grants. Federal money was especially important because it paid faculty salaries and graduate stipends *and* because overhead on federal grants paid for improvements in the university's infrastructure.[2]

Because of Terman's focus on federal grants, topics relevant to military technologies or matters of national security flourished at Stanford in the early Cold War because there was ample funding for those topics from the "mission-oriented" federal agencies. These agencies included the research funding arms of the military services; nominally civilian but strongly defense-oriented agencies such as the Atomic Energy Commission (AEC) and, later, NASA; intelligence organizations such as the Central Intelligence Agency (CIA) and the National Security Agency (NSA); and, after Sputnik, the Department of Defense's new high-risk research funder, the Defense Advanced Research Projects Agency (DARPA or, under some presidential administrations, ARPA).

Terman's strategy therefore required Stanford professors to appeal to the military-industrial complex. However, Terman didn't want Stanford researchers to compete with industry in developing new technologies. Nor did he favor faculty members' working on technological problems related to specific commercial products, which he thought would divert them from the more generalizable research that won federal grants and boosted academic rankings. Thus, Terman promoted the "basic research" end of the "linear model of innovation" that was becoming enshrined in federal science policy, in the Department of Defense's procurement accounting, in the corporate tax code, and in mainstream economic analysis.[3] Doing so, however, required a delicate balancing act. How could a scientist—and especially an academic engineering scientist—claim to be doing esoteric, fundamental, curiosity-driven, basic research *and* invoke the relevance of his or her work to national security?

No single stable answer to that question ever emerged. Different groups at different times have made do with *ad hoc* ways of squaring the circle, but such groups have always been challenged by other groups that have (or wish to have) more say in directing American science. In the late 1960s, new groups—especially the environmental, anti-war, and civil rights movements—began to see the reorganization of American science as an important front in their struggle to reorganize American society.[4] Thus, just when James Meindl arrived at Stanford in 1967, conflicts over the proper balance between basic and applied research intensified sharply.

MOS and the Optacon

As was true for many of his new colleagues, Meindl's views on basic and applied research stemmed from his experience in the private sector and in translating laboratory work into technologies in the field. As a researcher at Westinghouse in the late 1950s, Meindl worked on semiconductor rectifiers used in control rods for nuclear reactors. After he moved to Fort Monmouth in 1959, Meindl worked on low-power semiconductor circuits in his role as a researcher. He also helped make critical decisions about what external microelectronics research the Army should support to improve its arsenal. These experiences prepared him well for Stanford's Electrical Engineering Department, which teemed with men (overwhelmingly men) who had worked in corporate high-tech research organizations (especially Bell Laboratories) and who cultivated relationships with both industry and the military. Industrial experience and military-industrial relationships gave these academic physical and engineering scientists insight into how commercial technologies such as microelectronic circuits were developing, and therefore into how to insinuate Stanford into that process of development. Their backgrounds also gave Meindl and his colleagues a keen sense of what types of research industry could not or would not do at any particular moment, and therefore what niches were open to academic researchers who didn't want to compete with their industrial partners.

At Stanford, Meindl found just such a research niche with help from the chairman of his department, John Linvill. Linvill had come to Stanford from Bell Labs in 1955, and had proceeded to stock his department with industry veterans. Linvill was also an active institution-builder inside and outside Stanford. For instance, he was one of the founders of Stanford's Solid State Electronics Laboratory, and of the International Solid

State Circuits Conference and the Institute of Electrical and Electronics Engineers' Solid-State Circuits Council, which created a *Journal of Solid State Circuits* in 1966 with Meindl as its founding editor.[5] Meindl's earlier professional connections to Linvill made it more likely that he would receive, and be receptive to, an invitation to join the Stanford faculty, and his career path ensured that he would fit in with new departmental colleagues from similar backgrounds.

Meindl and Linvill soon became close friends rather than mere colleagues in large part because Meindl joined a project in which Linvill had a personal investment: the Optacon (OPtical-to-TActile CONverter). The Optacon was a device that allowed vision-impaired users to read text without converting it to Braille—the user simply moved a scanner over the text with one hand, while the other hand rested on a matrix of small pins that vibrated in a pattern that corresponded with the pattern imaged by the scanner. With training, an Optacon user could read text at "up to 90 words per minute, about half their Braille reading speed."[6]

Linvill freely admitted that his daughter Candace, who had been blind since she was eight months old, provided his initial motivation to pursue the Optacon.[7] The specific idea for a pin-based tactile reader, however, came to him during a sabbatical in Switzerland in 1962, when he "went to an IBM Research Center in Germany and saw a high-speed printer" that worked on a similar principle.[8] Linvill's sabbatical had been funded by the Office of Naval Research, and when he returned to Stanford he persuaded his ONR grant officer to "dig up modest amounts of money" to support development of the Optacon.[9]

The Navy complied, even though its interest in a reading aid for the blind was not self-evident. When asked later why ONR had supported the Optacon, Linvill gave multiple reasons: "[N]aval people in submarines need to communicate by touch as well as by vision,"[10] but also "ONR takes a broad view of its mission. They know that any work done with respect to a new form of communication probably has some future positive utility to the Navy. In addition … the Navy, along with others, was interested in aid for the blind because of their wounded veterans."[11] But in the early 1960s defense research funding was still unconstrained enough that ONR could bet on the Optacon without needing to specify exactly how it would benefit the Navy. Linvill's patrons knew that, at the very least, the project would help train master's and PhD candidates, who would then take jobs in government laboratories or with defense contractors and/or join the unofficial scientific reserve available during future national emergencies—an explicit aim of much of the military funding for basic academic research during the early days of the Cold War.[12] One might also speculate that Navy grants such as the initial one for the Optacon were facilitated by long-term personal relationships between faculty members and grant officers who could be persuaded to pay for small side projects. The Office of Naval Research may have perceived Linvill's personal enthusiasm for the project and diverted funding to keep a valued researcher happy.

Within months of starting work on the Optacon, Linvill met James Bliss, a researcher at the nearby Stanford Research Institute who was working on a tactile reader that used puffs of air instead of Linvill's vibrating pins. Like Linvill's grants, Bliss' grants came from national-security agencies, primarily NASA and the Air Force. Still, the Optacon's utility to the space race or the battlefield was dubious enough that Linvill and Bliss made only slow progress, even after they combined their grants. Initially, they decided just to "test whether or not a person could read letter shapes on an array of vibratory pins," and "the knowledge that persons can read such shapes was the first product," achieved in 1964.[13]

The first prototypes were the size of a desk and could scan one letter at a time off specially printed cards, so users were limited to between ten and twenty words per minute. Thus, the feasibility of the project depended on making the device faster. Linvill also seems to have aimed from the beginning for a device small enough for users to carry. Integrated circuits—with their potential to make devices faster and smaller—were one way forward. Integrated circuits were also becoming increasingly important to local industry and to electrical engineering more generally, and thus Linvill could see departmental, not just personal, reasons to build Stanford's capacity to fabricate ICs. As a first step, a member of the EE faculty, Robert Pritchard, "set up a demonstration lab on integrated circuits … to let people know what could be done and what the general steps were, but not to make working devices."[14] Getting from "demonstration" to "working devices" then fell to Meindl when he joined Stanford and assumed directorship of what came to be called the Integrated Circuits Laboratory (ICL).

Optacon circuits were among the first circuits to be fabricated in the ICL, and Meindl quickly became the third collaborator in the reading-aid project. Making and testing Optacon circuits proved a fruitful pedagogical task. By 1970 twenty to thirty graduate students had worked on the project in some capacity, and fifteen to twenty attended Bliss, Linvill, and Meindl's weekly joint group meetings.[15] As the project matured, those students and the equipment they used to fabricate Optacon circuits were increasingly funded from civilian rather than national-security sources. Much of the funding to set up the ICL, for instance, came from industry—particularly

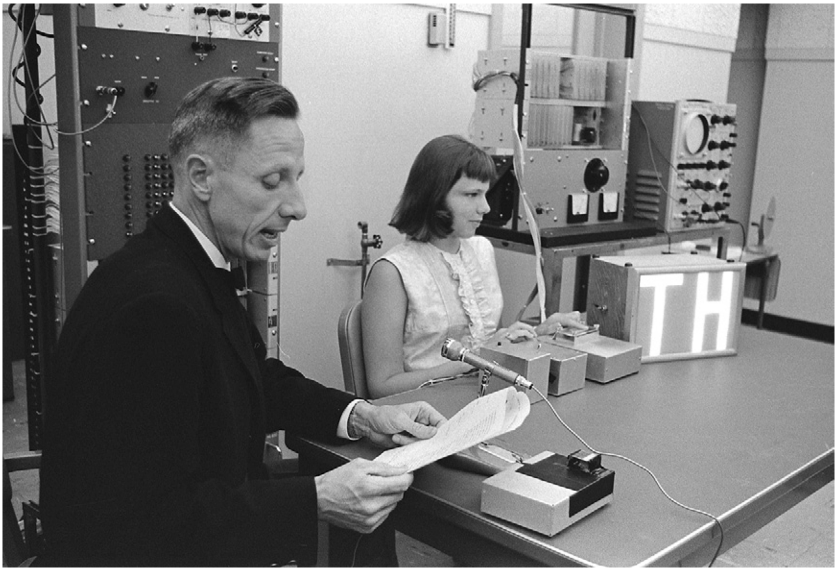


Figure 1.1
Candace Linvill demonstrating an early version of the Optacon while her father, John Linvill, explains its operation. Photo by Chuck Painter. Courtesy of Stanford News Service.

from Fairchild Semiconductor, the leader in moving US semiconductor firms away from military patronage and toward civilian markets.[16] Linvill and Meindl still needed federal support for the Optacon and the ICL, of course, but instead of seeking further funding from NASA or the ONR they turned to the National Science Foundation and the federal Office of Education.[17]

The major task supported by their Office of Education grant was to convert the Optacon from circuits based on bipolar junction transistors to circuits based on metal-oxide-semiconductor (MOS) transistors. Bipolar transistors allow for amplification of a signal or binary switching for logic through the movement of electronic "carriers" (electrons and "holes" which act like positive electrons). A small change in a control voltage or current results in a dramatic change in the flow of holes one way and electrons the other way, creating a large change in total current (an amplification) very quickly (a fast switching time for binary logic). In an MOS transistor, similar amplification and switching is achieved by applying an electric field to expand or constrict a channel through which one type of carrier (either electrons or holes) can move.

The bipolar transistor was at the time the overwhelmingly dominant type in commercial production. In view of the level of miniaturization achievable in the late 1960s, MOS transistors were still significantly slower than bipolar ones and were only haltingly going into mass production. However, bipolar transistors use more power and generate more waste heat than MOS transistors. Without Moore's Law bipolar transistors' greater waste heat would mean little, but if Moore's Law held true bipolar transistors would soon be packed close enough together that their waste heat would disrupt circuit operation. If the same miniaturization trend held for MOS, circuits containing MOS circuits would soon be almost as fast as circuits built around bipolar transistors, would consume less power, and would have greater potential for further miniaturization.

Established firms—even the ones that had invented or extensively developed MOS technology, such as RCA and IBM—were convinced that bipolar transistors were the technology to bet on, especially for computing applications.[18] The most important computer customers of the day wanted speed more than anything, especially for national-security-relevant applications such as cryptography and simulations of thermonuclear reactions. For those customers, mainframes based on bipolar transistors were favored over computers based on MOS circuits, and would be until the beginning of the 1990s. However, speed counted less for many civilian applications. Firms that focused on civilian customers, and especially start-up firms that couldn't compete in manufacturing bipolar transistors, were therefore more willing to explore the alternative path of MOS. That calculus was a big part of Gordon Moore and Robert Noyce's choice to transition their new firm, Intel, from products built around bipolar transistors to products based on MOS transistors in the late 1960s and the early 1970s.

As Meindl noted in 1967, "approximately 5 MOS devices can be packed into the same area taken by one bipolar transistor."[19] Smaller size and lower power consumption—but accompanied by lower speed—was not much of an advantage for mainframe computers, which sit in one spot and plug into the electrical grid. For applications in which a device had to move, be carried, and/or be disconnected from the grid, however, MOS circuits' low power consumption and small size were considered beneficial. Such applications included the digital watches and pocket calculators that were becoming important consumer products for Intel and other start-ups. Some military applications, too, had to be light in weight and to consume little power. Thus, the Army funded Meindl's work on MOS circuits in order to explore their potential use in communications devices in soldiers' and pilots' helmets.[20]

The Optacon also stood to benefit from small, low-power circuitry. As the team put it in 1970, "Current versions of the Optacon have been implemented with a monolithic bipolar phototransistor. … A new version … employs three custom MOS integrated circuits … , making it smaller, lower in cost, and simpler in construction; therefore, its utility is greatly enhanced."[21] As they gained proficiency in fabricating integrated MOS circuits, members of the Optacon team began to look to other civilian applications in which small, low-power circuits would be advantageous. In particular, Meindl explored a series of other disability technologies and biomedical instruments, including implantable ultrasonic blood flow meters, transducer arrays for monitoring blood pressure and muscular contraction, and mobility aids for the blind (to help users "see" and navigate around objects in space). Thanks to the Integrated Circuits Laboratory, other colleagues in Stanford's Department of Electrical Engineering, especially James Angell and Robert L. White, joined Meindl in developing low-power integrated circuits for neural prostheses (e.g., implanted hearing aids) and biomedical instrumentation.[22] Stanford's lead in conducting academic research on MOS circuits made it a center of innovation in biomedical and disability technologies, and its strength in research on biomedical and disability technologies gave it an edge in MOS that few universities could match.

Integrating Circuits and Disciplines

Applying MOS circuits to disability technologies and to biomedicine made obvious sense at Stanford in the years around 1970 in a way that was not as apparent a few years earlier or a few years later. The years when the Optacon was evolving most quickly were a period of disruptions in American science that threatened to derail institutions such as the Integrated Circuits Laboratory. One historically specific advantage of MOS circuits, therefore, was their "multivocality"—that is, their protean ability to appeal to multiple audiences with conflicting aims.[23] MOS could be plausibly connected to social betterment (through aiding the ill or disabled), to economic growth (when commercialized by firms like Intel), or to national security. Yet in the late 1960s MOS was still rudimentary enough that it was not *certainly* tied to any of these aims. Thus, groups such as Meindl's and Linvill's could get funding to explore MOS as an alternative applicable in any of these sectors, but could deflect criticism of such patronage by pointing to MOS transistors' other areas of application or by framing MOS development as basic research.

The multivocality of MOS was especially salient to Meindl and Linvill in the late 1960s because Stanford was then embroiled in campus unrest directed, sometimes violently, at the conduct of academic research. The historians Stuart W. Leslie, Rebecca Lowen, Matthew Wisnioski, and Eric Vettel have examined how Stanford and a few other leading research universities were besieged in the late 1960s by riots, by picketing, by takeovers, bombings, and arson of laboratories, and by humiliating pranks played on senior administrators, all in protest of academic scientists' and engineers' alleged complicity in the Southeast Asian conflict.[24] In a milder form, a much wider swath of students and faculty members registered their discomfort with the military-industrial-academic complex through teach-ins, reform-minded courses (often run by students), and achingly earnest discussions (sometimes in underground magazines founded specifically to promote reform). In that environment, the civilian applications of MOS were very attractive.

The multivocality of MOS was also useful because it gave Linvill and Meindl more options in dealing with a simultaneous budgetary crisis in American science. After explosive post-Sputnik growth, academic research funding from federal and state governments, from industry, and other sources remained flat in constant dollars from 1968 to 1976, then rose at a glacial pace until 1985, when the Reagan spending boom kicked in.[25] Federal research-and-development funding to all grantees (government, corporate, and academic) declined for ten years after 1967 and (adjusting for inflation) didn't return to their previous peak until 1985.[26] By facilitating their appeals to all kinds of funders—military and civilian, public and private—MOS technology increased Meindl and Linvill's share of a shrinking pie.

As that pie shrank, the job market for physical and engineering science graduates also dried up. In physics, the years around 1970 saw what David Kaiser has called "the worst job shortage the nation has ever seen—far more protracted than any employment-placement difficulties during the Depression years"; engineering graduates faced similarly dire prospects.[27] Both the lack of jobs and disaffection with the military-industrial complex meant that fewer young people enrolled in graduate and undergraduate programs in the physical and engineering sciences. Here, again, the multivocality of MOS technology helped, in that the PhD projects to which it could be applied would attract graduate students whether they were "doves" or "hawks," and the industries in which it could be applied were diverse enough that Meindl and Linvill's students' job prospects would be better than those of their peers.

The diverse applications of MOS technology also helped Meindl and Linvill satisfy growing demands for more, and more wide-ranging, interdisciplinary collaborations. The word "interdisciplinary" was on everyone's lips at Stanford in the years around 1970—in speeches by administrators, in campus newspaper articles, in grant proposals, in new interdisciplinary centers, in manifestos from the SDS, and elsewhere, and it was usually paired with one or another of the buzzwords of civilianization: "problem-oriented," "relevant," "national needs," "human problems," and so on.

In that context, MOS research stood out for its capacity to help electrical engineers forge ties with more civilian-oriented disciplines and funders. Indeed, even before the campus erupted in protest Linvill had begun arguing that his discipline should accommodate itself to more interdisciplinary and civilian-minded conditions. In 1967 he issued a memo responding to a request from Stanford's president that departments outline strategies for "studying and attacking the problems of today's society":

> Electrical Engineering, with its aim to bring technological tools to the solution of man's [*sic*] problems, is interested to join with other departments in working on these contemporary problems. … Engineering is distinct from pure science by its direction to application. The usefulness of university engineering and science to our defense efforts since the beginning of World War II is clear and well documented. It is also clear that attention should be directed in a university to other problems. The problems mentioned in the President's letter of November 8—those of environmental studies, urban problems, problems of developing countries, etc.—are timely problems to which the university in many of its parts should be directed. These problems cannot be attacked within a single discipline. Technology, by itself, is not sufficient. Multidisciplinary approaches must be used.[28]

Here we see Linvill establishing a template for a new kind of research that others could emulate—interdisciplinary, modeled on "defense efforts," but now applied to civilian problems. At the same time, he was putting that template into practice with the Optacon, which attracted faculty members and graduate students in mechanical engineering, psychology, neuroscience, and education (in addition to electrical engineering) and which had partially migrated from national-security funding sources to civilian ones.

Remember, though, that the Optacon hadn't begun as a model for "attacking the problems of today's society." The meaning of the reading aid changed as the environment with which it co-evolved changed. In the early 1960s, the Navy and NASA were willing to spend a little money on the Optacon on the assumption that the technologies and personnel that the project generated could be harnessed to national-security aims relatively easily. The Optacon itself, though, was not something to trumpet to mission-oriented funders. For instance, the descriptions of the reading aid in the annual Stanford University Electronics Research reviews distributed to funders (mostly national-security-oriented agencies) before 1969 were terse, dry, and buried at the back of the reports. The 1969 report, however, came out only four months after the student takeover of the Applied Electronics Laboratory and associated occupations and blockades of the Stanford central administration and the Stanford Research Institute (SRI). The need to show that Stanford electronics researchers were making positive contributions to civil society must have been intense. Thus, the authors of the 1969 review devoted page 1 to the Optacon, giving it a lengthy description in moralistic tones and emphasizing its role in moving Stanford away from military funders rather than its indirect benefits to the military services.

In particular, the report argued that

> Two nontechnical aspects of the reading-aid project are noteworthy. (1) Integrated circuits, principally developed to the present stage for space and military applications, are powerful tools for the solution of human problems, as this research project illustrates. (2) Such projects pose substantial technical problems in an area not presently of great commercial interest; they are ideally suited, however to graduate research topics, channeling interest of the imaginative graduate student to important social problems and providing a vehicle for interesting technical research.[29]

This passage lays out the multivocality of integrated circuits: Yes, ICs might have been “principally developed to the present stage for space and military applications,” but now they could be used “for the solution of human problems.” They could lure “the imaginative graduate student” who might otherwise have idealistic objections to working in the military-industrial complex, yet even so they could still be “a vehicle for interesting technical research” that might yet have military-industrial applications.

By the same token, when protesters and activists came calling, the multivocality of integrated circuits allowed Meindl to defuse criticism. For instance, when a student-run Stanford Workshop on Political and Social Issues (SWOPSI) examined “DOD Sponsored Research at Stanford” in 1970–71, it asked Meindl to explain the purpose of his “Micropower Integrated Circuits” grant from the Army. He responded as follows: “[T]he only application to date of the results of the Micropower Integrated Circuits contract has been accomplished at Stanford under NIH sponsorship for research on integrated circuits for medical applications.”[30]

Tying the Linear Model in Knots

Adapting to the budgetary, political, and cultural pressures of the late 1960s and the early 1970s was not merely a matter of deciding to work on new kinds of projects with new kinds of collaborators. Linvill, Meindl, and other academic researchers also needed to reconceptualize the intellectual relationship between basic and applied research, and the financial and administrative relationships among universities, firms, and government agencies. Note, for instance, Linvill and Meindl's ambivalence toward "basic" engineering research in the quotations in the preceding section. On the one hand, "engineering is distinct from pure science by its direction to application"; on the other, an Army-funded MOS project was "really looking at very basic questions on how to reduce the operating power level. … It's very basic research on devices and circuits." On their face, these statements are in tension even if it is presumably possible to construct a framework such that both statements would be analytically true. However, I would like readers to notice the reordering of the contexts in which Linvill and Meindl deployed notions of "basic" and "applied" and the ways in which MOS circuits aided them in that re-deployment.

In the early Cold War, researchers were encouraged to represent their work as "basic" when it was funded by civilian agencies, especially the National Science Foundation, in order to satisfy a standard of quality and objectivity determined by the peer review process. One purpose of that criterion was to prevent politicians (especially liberal ones) from using the NSF to support research in aid of their political aims.[31] The meritocratic criteria for NSF funding were also meant as an assurance that the NSF would continue to support high-quality but seemingly impractical research in, say, astronomy. Yet those same criteria made it awkward for the NSF to fund engineering and social scientists whose work was "distinct from pure science by its direction to application," even when those scientists claimed to be doing basic research.

Initially, the NSF simply did not have programs in the engineering and social sciences.[32] Instead, those fields found ready patrons in the military services and allied agencies (e.g., NASA) and philanthropies (e.g., the Ford Foundation). In the early Cold War regime, those entities were authorized to fund research that could be represented as either basic or applied, because their aim was to complete a mission integral to national security with whatever was at hand: personnel, prestige, and fundamental discoveries generated from basic research as well as techniques and technologies resulting from applied research.

In the early 1970s, though, federal agencies were forced to divide responsibility for basic and applied research in an entirely new way. On the one hand, the so-called Mansfield Amendment put the military temporarily out of the business of funding basic research. On the other, the American electorate demanded that civilian agencies turn toward "problems of today's society" rather than just answering fundamental questions of interest to an inward-looking scientific community. Thus, applied research was vigorously taken up by the era's new civilian agencies, such as the Environmental Protection Agency and the Energy Research and Development Agency. Likewise, the NSF created a Research Applied to National Needs program, which at one point was spending 10 percent of the foundation's portfolio on applied grants such as one to physicists at the State University of New York at Stony Brook to help New York City collect garbage more efficiently.[33] The NSF also elevated basic engineering and social science research programs to nearly the same status as existing programs in math and basic physical and (non-medical) life sciences.[34]

These shifts in federal practice and national attitudes put academic engineering scientists into a bind. As Meindl's example shows, when answering activists, it was wise to cast academic engineering research as "basic" or "fundamental"—but not too basic, because then he could be branded as doing "useless" research while ensconced in the ivory tower. Such criticisms could be defused by articulating applications of engineering science in obviously socially beneficial areas such as health care and assistance to the disabled. However, taking money from national-security agencies and then pointing to civilian technologies as the primary fruits of that funding could cause problems with grant officers at those agencies. Representing national-security-funded projects as primarily oriented toward basic or fundamental questions, meanwhile, could run afoul of the Mansfield Amendment.

Thus, the upended politics of basic and applied, military and civilian research around 1970 encouraged academic scientists and engineers to be flexible in how they represented their projects. Being *too* flexible, however, could draw accusations of gaming the system. Stanton Glantz, a Stanford engineering graduate student (later a prominent anti-smoking activist), put it this way:

> Under the current system, the faculty appears hypocritical to the point of dishonesty. Some of the faculty incessantly chortle about how they rewrite contract proposals to "get around the Mansfield Amendment" or "get the DOD to fund what I want." The fact remains that the Mansfield Ammendment [*sic*] is the law of the land, and lax enforcement aside, every citizen has an obligation to do his best to live up to the law.

Threading the needle to satisfy academic engineering science's warring constituencies was extraordinarily difficult in the early 1970s.

For John Linvill, the multivocality of electronics technologies was particularly well suited to balancing the demands of faculty members' different audiences. For instance, in 1970 he teased the faculty senate by asserting that the 1969 Stanford electronics research review "identifies the support agencies for the various projects, DoD agencies being shown with an asterisk." He continued: "I suggest a game … called 'Guess the Sponsor.' The aim is to identify the half of our research supported by the DoD, using the description of work actually done; I predict the reader will find the game difficult. The reason is that the research questions addressed by our research projects typically are now broad and of basic nature."[35] Nevertheless, Linvill admitted that the civilianization of his department's research was going to shift faculty members in a more applied and perhaps less flexible direction because declining defense budgets would force them to seek funders who were less interested in basic research: "New research programs, particularly new research sponsors, ordinarily have specific problems to solve. At this point as we seek additional research support, we will tend to find applied problems."[36]

University-Industry Linkage in the Crisis Era

Among those "new research sponsors," commercial firms were set to become ever more important patrons for academic electrical engineers. Recall that Stanford's postwar research enterprise had been constructed so that faculty members would aid, and not compete with, industry. Since the late 1960s, however, that research enterprise has been squeezed financially, politically, and culturally toward applied projects that could potentially overlap—and thereby compete—with industry. Making applied academic research complementary to commercial research and development, rather than competing with it, was another needle that could be threaded several different ways, all of which involved blurring lines between university and industry.

One long-established link between Stanford and its commercial patrons was the training of students for industrial jobs, often in companies that paid to be Stanford's "industrial affiliates" in return for the right to scout the campus for promising recruits. In the early Cold War, engineering education (both graduate and undergraduate) had emphasized fundamental knowledge in part to give students a maximally flexible skill set suited to the largest number of job openings. By the late 1960s, though, many students were demanding opportunities to work on socially beneficial applications in the course of their education. Indeed, Linvill made satisfying those demands a departmental priority: “Specific tasks to be performed by faculty and students will be identified by those wishing to undertake research directed to these societal missions.”[37]

How, though, to train students on specific applications without stepping on the toes of industrial patrons? The answer was to direct students toward technological platforms that weren’t yet commercial mainstays, but which firms were considering adopting in the future. Low-power MOS integrated circuits fit this bill perfectly: Intel and some other firms were just beginning to use them for memory circuits, while Stanford students could apply them to biomedical and disability technologies that satisfied their idealism but weren’t ready for the market. A 1973 pamphlet for Meindl’s Integrated Circuits Laboratory put it this way:

> During the past decade, integrated circuit technology has had a revolutionary impact … ranging from complex computer and communications systems to electronic wristwatches and pocket calculators. The central objective of the courses and research program of the Integrated Circuits Laboratory is to prepare the student to use integrated circuit technology in an innovative manner in solving the problems of our society. … The research program of the ICL provides a broad opportunity for fabrication, analysis, design and application of novel integrated circuits for systems of importance to our society particularly in the field of medical electronics [such as] … a new reading aid for the blind; … chronically implantable blood flowmeters; … an ultrasonic image sensor for use in echocardiography and soft tissue imaging; and novel silicon pressure diaphragms … for use in research on the reproductive process. … Frequently, a team approach to a timely specific project is employed. The reality that results is an effective means for transition from the highly academic environment to the ‘real world.’ A result of this may be some deferral in choice of a dissertation topic in order to choose one that emphasizes more practical rather than theoretical significance.[38]

Note the repeated invocation of “solving problems of our society” of “practical rather than theoretical significance.” Note too that applicants were told of integrated circuits’ market impact, but then reassured that their graduate training would focus on entirely different, less commercial applications. This framing of academic electrical engineering research was prominent during the peak years of student activism. Once military conscription ended and campus protests diminished, however, the ICL was reframed as a place where contact with industry took priority over “solving the problems of our society.”

Focusing the Integrated Circuits Laboratory on socially relevant applications of low-power circuits attracted graduate students without stepping on industry's toes, but it also created a dilemma: If no firm would commercialize the technologies coming out of Meindl's lab, then how could those technologies actually contribute to "solving the problems of our society?" The answer that Linvill, Bliss, and Meindl drifted into was to connect to the market directly by patenting their work and founding their own firm to license the patents. Of course, they weren't entirely alone in making this choice, though they were ahead of the curve. As a number of studies have observed, the 1970s saw a marked increase in the rate of faculty patenting and academic entrepreneurship in the United States.[39] Indeed, Stanford was an important contributor to that trend, especially after the founding of its Office of Technology Licensing in 1970.

Today, Stanford's Office of Technology Licensing and its director, Niels Reimers, are much honored for their role in patenting recombinant DNA and contributing to the formation of the biotech industry at the end of the 1970s.[40] Reimers' later fame in biotechnology, however, inhibits understanding the variety, purpose, and success of patenting and entrepreneurship at Stanford at the beginning of that decade. For one thing, the political, cultural, and budgetary crises afflicting Stanford are underplayed in stories about Reimers and his office. While I would not argue that those crises *led* to the founding of the OTL, there is good evidence that the OTL aimed to make up for reduced federal research funding and to demonstrate that the university was not indifferent to society's problems. As Reimers himself put it in 1975, "what I'm supposed to do [is] marketing Stanford's technology to industry for public use and benefit and to derive income to reduce the cost of education."[41] Upper administrators concurred, at least in public, that civilian benefit was an important justification for the OTL. In 1970 a Stanford vice president put it this way: "I am pleased by the high societal value of inventions we have licensed [such as] a potential cure for viral infections and a potential ecologically safe insect control."[42] After all, if an on-campus discovery could be brought to the open, civilian market, associating it with the military-industrial complex would be more difficult.

Both with and without the OTL's help, Stanford faculty members in many departments—not just the life sciences—were patenting more frequently from the late 1960s on. Electronic technologies were especially fruitful, both in numbers and in quality of patents. Andrew Nelson, for instance, has tirelessly pointed out that one of the most valuable patents in Stanford's portfolio (generating nearly $23 million) was John Chowning's invention of FM synthesis for electronic music.[43] Patents such as Chowning's make sense only in the context of a turn toward civilian applications and a vogue for interdisciplinary collaborations. Chowning's FM synthesis circuits were an electronic technology, but Chowning himself was located in the Music Department and in a new Center for Computer Research in Music and Acoustics linking Music with Computer Science, Electrical Engineering, Psychology, and other departments. As Nelson and I have argued, the CCRMA provided humanities fields with opportunities for grants on the model of the sciences and engineering, but it also provided science and engineering fields with opportunities to move away from dependence on military funding.[44]

Linvill and Meindl's steps toward participation in the market, too, were marked by both a wide-ranging interdisciplinarity and a civilian, humanistic outlook. Their first patents as academics (both had patents from their pre-Stanford employment) were for the Optacon and an "ultrasonic blood vessel imaging system."[45] When they couldn't find anyone to license the Optacon patent, they co-founded Telesensory Systems, Inc. (TSI) with James Bliss in 1970, Bliss leaving SRI to run the company. Entrepreneurship, like the Optacon itself, was framed as part of the wider attack on "problems of our society." As Telesensory Systems' business plan explicitly stated,

> A need exists for manufacture and marketing of products and associated services arising from certain research projects carried out at Stanford University and Stanford Research Institute. These research projects (e.g., the Optacon) are in socially beneficial areas, biomedical engineering in particular, that have been funded by the federal government. The most effective way to realize the fruits of this type research will be through a private organization, such as Telesensory Systems, Inc., that has close ties in such a way that they are of mutual benefit to the three groups involved.[46]

And, indeed, Telesensory Systems did turn out to be an "effective way to realize the fruits" of ICL research, since eventually it got "over 10,000 Optacons manufactured and into the hands of blind people."[47]

Thus, at least in the case of the Optacon, academic patenting and entrepreneurship were necessary to make socially relevant research actually relevant to society. Moreover, it would be hard to argue from the Optacon case that commercialization was deleterious to academic science. In fact, by working on and then commercializing civilian-oriented projects such as the Optacon, Linvill and Meindl legitimized academic engineering science and drew in students who were disenchanted with the military-industrial complex. In 2011, in a memorial statement for John Linvill, Stephen Brugler (one of the Optacon graduate students who went to work for Telesensory Systems) said "I'm especially thankful for [Linvill's] willingness to have

Stanford pay off my obligation to Uncle Sam, thereby saving me from a career in weapons development."[48]

Still, the TSI founders' frequent nods to market morality signaled their turn—and that of American academic science more generally—toward the needs of industry rather than society from the second half of the 1970s on. For instance, Linvill argued that "openness of TSI to competition is insurance to the blindness system of economical instruments."[49] He also implied that competition "in the best sense of American free enterprise"[50] was what made TSI both technologically and morally superior to non-market means of aiding the blind: Linvill's "idea was people and profits, not people or profits; it was to apply Silicon Valley innovation to products for the blind and handicapped. It was a new approach to an area historically dominated by charity and we showed that it could work."[51] As Linvill himself said at the time, "Education of the blind is not a matter for charity, I profoundly believe."[52] Why? Because economic metrics—such as increased tax revenue from helping Optacon users find jobs—would suffice to justify the project.[53]

Yet until the late 1970s Linvill's invocation of metrics and profits probably stemmed from his quantitative and business-oriented background as a former corporate engineering scientist, rather than from a commitment to full-throated market fundamentalism. Linvill's lack of faith in a neoliberal view of the market is particularly underlined by his frequent invocations of the Optacon as good evidence for a positive role for civilian government in academic science: "government, university, and industry are symbiotic in attacking blindness with technology."[54] Indeed, neither the Optacon nor TSI would have existed without extremely close ties among university, industry, and government. As we have seen, Optacon development at Stanford required inputs from firms and government agencies. The same applied to TSI, the seed funding for which included both a $25,000 investment from Stanford and an initial order for fifty Optacons (at $5,000 apiece) from the US Department of Health, Education, and Welfare.[55]

Solving Society's Problems, or the University's, or Industry's?

From the perspective of Stanford University's administration, the social relevance of the Optacon and Meindl's various biomedical applications of MOS circuits was invaluable in showing audiences inside and outside the university that Stanford's faculty members were in step with the times. For instance, when a faculty committee organized a 1971 research "exposition extending over the entire campus community. Interdisciplinary activities particularly are desired,"[56] the exposition highlighted "the use of holography (three-dimensional laser holography) in displaying art objects, a continuous interactive opinion poll with a computer terminal, the use of electronic devices to help the blind and to measure medical data inside the body, and a presentation of computer-composed music."[57] That is, Linvill and Meindl's projects ("electronic devices to help the blind and to measure medical data inside the body") were among a handful of much-touted examples used to advertise Stanford researchers' ability to cross disciplinary lines and address civilian problems. The MOS projects also demonstrated Stanford's ability to reach out of the ivory tower and make contact with civil society. Optacon use was taught in several elementary and high schools, first in California and eventually around the world, and the technology was featured in *Time*, on CBS News, and in an exhibit at the new San Francisco Exploratorium.

By 1975, however, Stanford's need for good publicity and diversified funding from MOS research had diminished. The crises besetting American science were over or had mutated into new forms. Student activist pressure on academic researchers evaporated as US forces pulled out of Vietnam. The federal research funding situation was still dire, but wasn't quite in the disarray of a few years earlier. Americans were no longer quite as disenchanted with science as they had been during the Vietnam era, as evidenced (and driven) by popular books such as *The Tao of Physics*.[58] As early as 1973, there were significantly fewer incentives to experiment with new ways for science to solve the problems of today's society, and hence scientists' and engineers' enthusiasm for such experiments (and their ability to sustain them) diminished.

As "stagflation" and economic malaise sapped politicians' will to fund research into disability technologies, federal funding for the Optacon was cut. As Linvill reported to Stanford's upper administration,

> The Optacon Project is now unfunded, and we are operating on a continuation-without-additional-funds request. … If any of you have contacts in Washington which might benefit my case, I will appreciate your support through them. Given the present budget condition and the low credibility of technology at OE [Office of Education], the case at best is tough.[59]

Linvill's frustration with the whims of federal funding comes through quite clearly in the following passage:

> I have never approached foundations for support, preferring to "sell" OE and then give them more than their money's worth. Education of the blind is not a matter for charity, I profoundly believe. However, I see no alternative which can

> solve our continuation problem in the short term unless OE moves with unrecognizable speed.[60]

The end of federal funding for the Optacon and the project's move away from the university and into the private sector were symptomatic of a broader turn toward partnerships with industry and away from the applications, funders, and interdisciplinary collaborations that academic engineering scientists had sought out in the early 1970s. As we will see in the next three chapters, the external shock that contributed most to this turn was the Japanese government's announcement of a crash program to improve Japanese microelectronics firms' capabilities in very-large-scale integrated (VLSI) circuits. That prospect transformed American policy makers' and semiconductor industry leaders' nascent worries about the rising market share of Japanese firms into a frantic panic and led to a cascade of new experiments in the organization of American science.

Of course, industry had to some extent been a party to the research reforms of the early 1970s, but with a different emphasis than later in the decade. Until 1973 or so, academic engineering scientists such as Linvill largely framed their work with industry as a matter of generating personnel, not discoveries. Academic research itself was for "attacking the problems of today's society," and *not* for attacking the problems of today's firms. In 1973, reporting to the Office of Education, Linvill said:

> The leaders of the semiconductor industry, many of whom are in this geographical area, were aware of our work at every step. … Continuing liaison with the semiconductor industry has been crucial. The Ph.D.'s produced, a co-product of the Stanford Optacon project, are of great interest as employees of the semiconductor industry.[61]

In this early-1970s configuration, universities contributed people, not discoveries, to industry, and industry gave back donations of equipment, materials, and know-how. For instance, one of the first graduate students to work on the Optacon was a Hewlett-Packard employee getting his master's degree.[62] The project's biggest corporate patron, Fairchild Semiconductor, not only supplied money and equipment for the Integrated Circuits Laboratory, but "made a special form of their commercial phototransistors" for the Optacon at the request of the Stanford-SRI team.[63]

Such arrangements weren't unusual. Christophe Lécuyer has documented extensively how people, ideas, and artifacts from the semiconductor industry flowed into Stanford for fifty years, while a much smaller flow of discoveries moved from Stanford out to Silicon Valley.[64] Nearby firms seem to have been especially willing to make and/or give small batches of their products to Stanford researchers for experiments that might lead to the opening up of an unexpected market with high publicity value—say, reading aids for the blind or chips for electronic synthesizers.[65] In fact, in 1972 Hewlett-Packard featured the Optacon in advertisements to reassure the weary readers of *Science* and *Scientific American* that "some things are changing for the better."[66]

For the better or not, things *were* changing. When we return to Linvill and Meindl's late 1970s institutional experiments in chapter 4, we'll see that they reversed several features that had characterized their relationships with government and industry in the early 1970s. Much later, Linvill explained:

> I once talked to Robert Noyce about how to foster industry support of university research. He said Gresham's law also applies to industry support of university research. As long as you can get something free from the government, Gresham's law says you'll never get it from industry. Cheap money always drives out dear money; that's Gresham's law. With constriction of government funding, "cheap" money from the government is disappearing, so academia must work with industry.[67]

As government money became less "cheap," the Optacon largely disappeared from Stanford; and while Meindl's biomedical device research lingered on, his primary focus shifted in the 1970s and the 1980s to "rapid turnaround" device fabrication in the Integrated Circuits Laboratory. Stanford's capacity to fabricate large numbers of integrated circuits meant that it no longer depended as much on gifts of devices from industry—but maintaining that capacity meant becoming more dependent on gifts of money from industry. That, in turn, meant a more cautious approach to intellectual property and open publication as industry expected something in return for its "dear" money: "academia must provide services of value to industry."[68] Discoveries, and not just personnel, were now expected to move from university to industry, and academic institutions such as the Integrated Circuits Lab had to adapt to that new currency of exchange.

But if the objective of institutional experiments such as the ICL shifted in the mid 1970s from "solving the problems of today's society" to solving the problems of industry and the American economy, the experiments themselves endured. As we'll see in chapters 4 and 5, the Integrated Circuits Laboratory was much copied in the 1980s because of, not despite, its retreat from developing hearing aids and reading aids for the blind. Of course, not everyone would agree there was a distinction between "solving the problems of our society" and solving the problems of firms. As economic ideas came to dominate public discourse, the gap between the needs of firms and those of society narrowed, at least in the context of American science and science policy. In 1973, Gordon Moore himself argued “we are really the revolutionaries in the world today—not the kids with the long hair and beards who were wrecking the schools a few years ago.”[69] Maybe so. But the “revolutionaries” of Silicon Valley do seem to have been less encouraging of projects such as the Optacon once pressure from the “kids with the long hair and beards” let up. Still, the brief flourishing of such projects deserves note, because they opened the door for the institutional experiments and departures from the early Cold War paradigm that are still thriving forty years later.

2 IBM Gives Josephson Computing a Try

The fundamental carrier of signals and information in micro*electron*ics is, as the name implies, the electron.[1] Solid-state electronic devices depend on the ability of materials to move electrons from one place to another or to block that movement with an energetic "barrier." Most devices contain components (transistors, resistors, capacitors, inductors, etc.) built from tiny structures composed of three classes of materials: conductors, insulators, and semiconductors. Conductors allow electrons to move with little resistance. Conversely, insulators impede electrons from moving. In a semiconductor, electrons with energies that fall within a certain range move as though moving through a conductor, whereas electrons with energies outside that range act like electrons in an insulator.

Electron-transporting materials in a fourth class are not employed in conventional microelectronics. Superconductors are materials that may be conductors, insulators, or semiconductors at room temperature, but which suddenly lose all electrical resistance below some very cold threshold temperature. This bizarre phenomenon is still not fully understood, and thus superconductivity has been a fruitful site of basic research ever since its discovery in 1911. Yet ever since the very beginning of solid-state electronics superconductivity has also held out the tantalizing promise of fast, low-power, loss-less circuits, and thus it has been a site of much applied research as well. A few niche applications of superconducting electronics, such as magnetic-resonance imaging machines, have reached the market in a process that superficially resembles a progression from basic discovery to applied development. Many more superconducting electronic devices have been proposed but have failed to reach practical application. Some of these impractical superconducting devices have been reborn as tools and sites of discovery—that is, have "progressed" from the domain of applied development to that of basic research. Taken together, these two potentialities latent in any alternative electronics (to move both/either from basic to applied and/or from applied to basic) constitute what I will call a “zig-zag” model of innovation.

The most ambitious end goal for applying superconducting electronics has been to build a commercially viable computer from superconducting circuits and thereby to wreak creative destruction on the entire semiconductor industry. A few organizations in the United States and in Japan have sponsored multi-million-dollar programs in pursuit of that prize. Seemingly, little has come from these programs, other than a few prototypes and a reinforced sense of silicon’s dominance.

This chapter examines the most substantial American venture into superconducting electronics: a twenty-year effort at IBM to build a computer based on the “Josephson junction.” IBM never sold a superconducting computer, so on its face the Josephson project was a technological failure. Yet I will argue that the Josephson program—like much of IBM’s research activity—made several indirect contributions to the firm, to its industry, and to science in general.

Moreover, IBM’s Josephson computing program participated in a profound transformation of relations among the microelectronics industry, American science, and the American state. In the mid 1950s, when superconducting microelectronics was first invented, the US national-security state still dominated the market for solid-state circuits. The microelectronics industry’s increasing focus on civilian markets, and the budget constraints on the military services that we saw affecting Stanford in chapter 1, meant that the US government had less leverage over semiconductor firms from the late 1960s on—just when IBM began to take Josephson computing seriously. That coincidence of timing means that the evolution of superconducting electronics can help us understand the federal government’s shift from an unchallenged shaper of markets for microelectronics development to a thrifty host of networks of microelectronics researchers.

Theory Made Real

In the early 1960s there was intense interest in superconducting materials. On the theoretical side, the Bardeen-Cooper-Schrieffer (BCS) theory of 1957 offered the first good explanation for superconductivity in certain materials (later known as Type I superconductors). Around the same time, Alexei Abrikosov in the Soviet Union identified another class, Type II superconductors, for which BCS theory fell short, therefore opening further vistas for theorists.[2] On the experimental side, several investigators—particularly at General Electric and at the Ford Motor Company—were pioneering techniques for growing thin films of superconductors that promised to lead to new commercial applications and to new tools for fundamental research.[3]

Thus, when Philip Anderson, a theorist of condensed matter from Bell Laboratories, spent a sabbatical leave at the University of Cambridge in 1961–62, he faced a classroom of graduate students eager to learn about recent developments in superconductivity. One student, though, was different: a brusque, awkward, brilliant 22-year-old named Brian Josephson. Anderson quickly learned that Josephson was a challenging classroom presence—"everything had to be right or he would come up and explain it to me after class," he later recalled.[4]

Josephson soon approached Anderson with some suggestive calculations he had made regarding how electrons in superconducting materials would behave in various configurations. What if you separated two chunks of superconducting material with a very thin layer of insulating material? What if you formed a piece of superconducting material into a loop, but you severed the loop with a thin insulating barrier? What if you placed a voltage across such a barrier? In other words, what if you made a circuit partly out of superconducting materials? What would happen to electrons as they traveled through that circuit? What could you *do* with such a circuit?

The mathematical aspects of Josephson's theory were formidable, even for Anderson. But Anderson soon became a champion of the graduate student's unorthodox insights. Two of those insights would prove particularly compelling to researchers dreaming of an alternative form of microelectronics and computing. First, Josephson saw that if one separated two superconductors with a thin enough barrier, then pairs of electrons would have some probability of quantum-mechanically "tunneling" across the barrier together. This was important because paired electrons are the carriers of superconducting currents, whereas normal currents are carried by single electrons and holes. Thus, a thin enough barrier would give rise to a supercurrent through the barrier even if no voltage were applied between the two superconductors—so long as nothing interfered with that supercurrent.

Josephson's second set of insights dealt with the conditions that *could* interfere with that supercurrent. He saw that one could, for instance, place a voltage across the barrier, in which case the supercurrent would become an alternating current with an extraordinarily high frequency. Or one could introduce a magnetic field into the circuit, causing a spatial variation in the supercurrent. In that case, some of the current would be moving in one direction across the barrier, while some might be moving in *either* the same direction or the opposite direction, depending on the strength of the magnetic field. That is, a very small difference in the applied magnetic field would cause the supercurrent to switch between a maximum (all of the supercurrent moving in the same direction) and a minimum (half the supercurrent moving one direction and half in the other). To those encountering Josephson's ideas with computing applications in mind, that behavior looked like a switch between a 1 and a 0.

His sabbatical over, Anderson returned to Bell Laboratories. By late 1962 his experimentalist colleague John Rowell had fabricated a device with which Josephson's and Anderson's predictions could be confirmed simply by "slid[ing] a bar magnet along the surface of a table towards the sample."[5] Other corporate labs also quickly took up Josephson's ideas. In 1964, for instance, Ford researchers put two insulating barriers into a superconducting loop to make what is now known as a superconducting quantum interference device (SQUID)—an invention with applications in neuroscience, metrology, and submarine detection.[6]

Superconducting electronics was exactly the kind of topic Cold War corporate research laboratories were designed to pursue. As Stuart W. Leslie has shown, many of America's leading high-tech firms in the 1950s and the early 1960s employed prominent architects such as Eero Saarinen to build vast, glamorous research centers in picturesque rural and semi-rural locales, often with the explicit aim of removing scientists from the "distractions" of product development and manufacturing.[7] Within these labs, the great multi-division firms of the era, such as IBM, AT&T, RCA, GE, Ford, and Westinghouse, sponsored a good deal of fundamental research, much of it in areas of seemingly little technological relevance—such as Arno Penzias' work at Bell Labs on the cosmic background radiation and Richard Garwin's research on gravitational radiation at IBM.[8] Firms did, of course, get a few tangible returns for their basic research—such as tax breaks and the possibility of external grants. Luminaries such as Penzias, Anderson, and Garwin also helped their employers by shaping federal science policy, bringing good PR to the company (Penzias and Anderson eventually won Nobel Prizes), and aiding their more applied colleagues in solving technological problems. Sometimes they managed more applied groups themselves. Plus, there was the slim but nonzero possibility that curiosity-driven research could yield insights that would revolutionize their firms' business.[9]

Superconductivity was exactly the kind of topic that allowed corporate researchers to pursue basic research while maximizing the firm's gains from that research. It was a deeply strange phenomenon, bound to capture the curiosity of the best and brightest corporate researchers. But it was also closely related to phenomena of obvious technological relevance, such as magnetism—meaning that curiosity about superconductivity might yield a faster return on investment than, say, gravity research. And, as the Ford team found out with its SQUIDs, superconductivity was weird enough that its applications might be impossible to predict. Indeed, the Ford team's invention of the SQUID has been of enormous public benefit—though its commercial relevance to Ford Motor Company was and is mysterious.

From the Ashes

One corporate lab was particularly well positioned to take advantage of Josephson's ideas, not because it had a record of success with superconductivity, but because it had a record of failure. In the late 1950s, IBM's flagship laboratory at Yorktown Heights, New York—officially the Thomas J. Watson Research Center, but informally known as IBM Yorktown—had applied considerable expertise and resources to an earlier superconducting device called the cryotron. Those resources later became available, serendipitously, for Josephson computing.

The cryotron predated Josephson's ideas, but took advantage of phenomena similar to the ones he predicted. Most cryotrons consisted of one superconducting wire wrapped around another; when current passed through the coiled wire, it created a magnetic field that quenched the supercurrent in the straight wire. In other words, a small change in the control current through the coiled wire resulted in a large change in the current through the straight wire, allowing the device as a whole to act as a switch or amplifier roughly equivalent to a vacuum tube or transistor.

The cryotron isn't well remembered today, but there is a good case that the world's first integrated circuit contained a superconducting cryotron rather than a semiconducting transistor.[10] Conventional histories of microelectronics credit either Robert Noyce of Fairchild Semiconductor or Jack Kilby of Texas Instruments with inventing the IC, but in fact it is difficult to come up with a definition of "integrated circuit" that encompasses Noyce and Kilby's semiconductor devices but not the earlier cryotron integrated circuits. Cryotron proponents were also some of the first to develop electron-beam lithography, one of the advanced techniques for fabricating ultraminiaturized circuits that we will encounter in later chapters.[11] The cryotron also shared important military-industrial patrons, especially the National Security Agency, with later efforts in Josephson computing.

Big companies, especially IBM and General Electric, seem to have become interested in the cryotron in late 1955, a little more than two years after its invention at the Massachusetts Institute of Technology. As figure 2.1 shows,

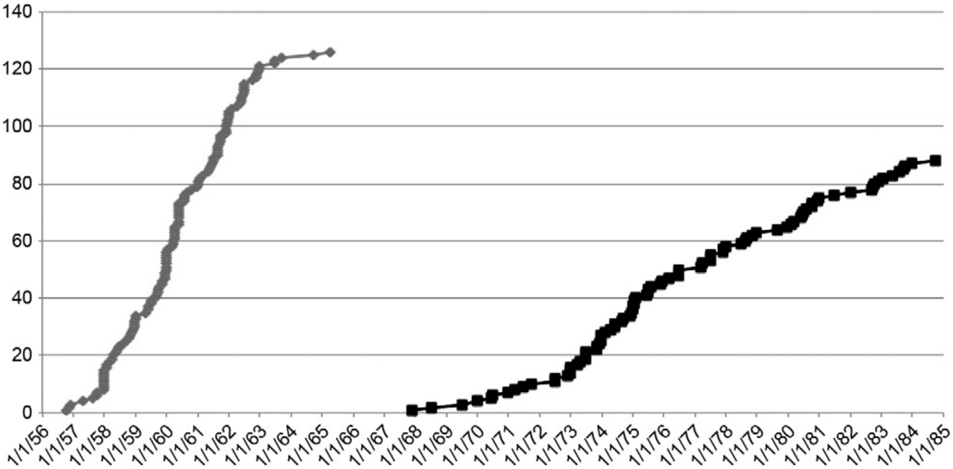


Figure 2.1

Cumulative number of patents over time for the IBM cryotron (left) and Josephson computing (right) programs. Counts for the cryotron were obtained by simply searching for patents assigned to IBM containing the word "cryotron." Counts for the Josephson program were obtained by means of a "snowball" methodology of inspecting all IBM-assigned patents for which the inventor was a known member of the Josephson program, then reiterating the search for patents invented by co-patentees of the known team members. Eventually a closed thicket was obtained.

IBM made rapid progress once it had committed to the cryotron—especially from 1958 to the end of 1962. Richard Garwin later estimated that about a hundred IBM employees were working on the cryotron in the late 1950s.[12] In the end, though, IBM and everyone else concluded that there were more promising alternatives that didn't involve superconductivity. As one GE participant put it, "realizing that Fairchild or others would eventually get Jean Hoerni's planar silicon transistor—which, unlike cryotrons, worked at room temperature—to work in large arrays, I could see the end of cryotrons."[13]

In calculations of this kind we can see the very beginnings of the entrenchment of Moore's Law as a social fact. In the early 1960s, companies that were weighing silicon transistors versus alternatives such as the cryotron were just beginning to conclude that silicon would eventually prove more amenable to making large numbers of logic elements than any other material. That conclusion was based in part on the physical nature of silicon (and its oxide, SiO_2, which grows easily on silicon and makes an excellent insulating barrier). But silicon's advantages were also self-reinforcing, in that the workforce innovating silicon manufacturing increasingly dwarfed the workforce dedicated to alternative microelectronics technologies simply because silicon's workforce had already discovered enough about silicon to make it an easier material to work with and one with a seemingly rosier future. Gordon Moore's 1965 "law" quantified the grounds for optimism about silicon, but that optimism was already palpable, if self-fulfilling, in the earlier decisions of firms and researchers to abandon non-silicon microelectronic devices such as the cryotron.

By late 1962, therefore, the IBM cryotron team's activities were tailing off, their equipment now unused and considered surplus. But then came a curious resurgence that, twenty years later, would temporarily put the status of Moore's Law as a social fact in doubt. That resurgence began in 1964 when IBM moved many of its Yorktown magnetics researchers to California to be closer to the company's facilities for manufacturing data-storage equipment. At that time, data storage meant magnetic storage, so this was a logical move. But a few magnetics researchers who remained at Yorktown persuaded management not to ship them west. In most companies, such stubbornness would be grounds for dismissal. IBM, though, was large enough—and indulgent enough toward its employees—to grant the request.[14]

Still, IBM had to find tasks for the rump Yorktown magnetics group. Eventually, a research manager named Philip Seiden proposed turning these five or six people into a superconductivity group, despite their lack of expertise in that area. That mattered little in an environment where smart people were expected to range widely and learn new fields quickly. Moreover, magnetism and superconductivity are related phenomena. In some ways, the fundamental characteristic of a superconductor is not that electrons move without resistance but that magnetic flux lines are expelled from a material when it is in the superconducting state.

While giving himself a crash course in superconductivity, one member of the group, Juri Matisoo, came across Josephson's new theory and soon concluded that he could use that theory to make microelectronic devices such as flip-flops or registers—some of the fundamental building blocks of a computer. In order to implement Josephson's ideas, though, Matisoo needed specialized equipment for growing thin films. Luckily for him, the cryotron project's old evaporators had been stashed in a corner of the Yorktown lab. Matisoo appropriated the equipment and began growing thin films of aluminum, tin, and lead. By April of 1965, he had filed an invention disclosure for a "cryotron-like logic element utilizing weakly-coupled [i.e., Josephson-based] superconductors."[15] As it turned out, another IBM employee, Marshal Merriam, had filed a similar IBM Technical Disclosure Bulletin almost a year earlier, so Matisoo had to abandon his.[16] Nevertheless, his supervisors encouraged Matisoo to continue working on logic elements based on the Josephson effect. However, one of them, Wilhelm Anacker, suggested that the name "cryotron" should be avoided, since it was a "failed technology." And so the tunneling cryotron became the "Josephson junction."[17]

The Zig-Zag Model of Innovation

The case of the cryotron and the "tunneling cryotron" highlights the omnidirectional relationship between basic and applied research in the practice of corporate labs during the early Cold War. This contrasts with the unidirectional "linear model of innovation," which historians have shown was an influential ideology in those corporate labs and in military-funded research during the same period.[18] The linear model posits a spectrum of inquiry: basic research, applied research, engineering science, technological development, and manufacturing. It is a unidirectional model because ideas are pictured as moving from the basic end toward the applied end; a grounding in fundamental knowledge is imagined as necessary to progress toward a working technology. Research becomes increasingly focused, particular, and driven by specific technological needs as it moves toward the applied end; while at the basic end inquiry is ostensibly more curiosity-driven, generalizable, and imagined as threatened by technological or commercial motivations.

Some features of the great corporate research centers of the 1950s and the early 1960s are hard to make sense of without some reference to the linear model. For instance, the leading corporate labs of that era began hiring many more people directly out of PhD programs, and retreated from their earlier practice of promoting non-PhD personnel into research from the shop floor. Big firms also built isolated research campuses like IBM Yorktown to insulate researchers geographically, organizationally, and intellectually from quotidian problem solving in manufacturing. Within such labs, the incentive structure rewarded publication of fundamental research at least as well as it rewarded contributions to the firm's products and processes.

Yet the observation that early postwar corporate research was *shaped* or *influenced by* the linear model does not mean that corporate scientists and research managers ever *adhered to* the linear model. Rather, the Josephson junction program at IBM and projects like it indicate that implementation of the linear model was patchy. The innovation model Matisoo and his colleagues practiced looked less like a unidirectional straight line and more like what I would dub the "zig-zag" model of innovation.

The essence of the zig-zag model is that any discovery or invention opens up new horizons of activity that can be perceived or interpreted as lying at any point on the spectrum from basic to applied.[19] Usually, a new insight reveals multiple possibilities containing various admixtures of "applied-ness" versus "fundamental-ness." Thus, in the zig-zag model there is no unidirectional flow of research from basic to applied. Instead, a variety of considerations helps scientists and engineers decide in which direction to move next. Policies and mores can load the dice to *favor* moving from basic toward applied, but at large postwar corporate labs the dice weren't always loaded that way. A technological effort such as the cryotron project could accrue equipment that might later be used to test Brian Josephson's exotic ideas. Out of those tests emerged patents and the broad outlines of a future technology *as well as* a basic research program in superconducting thin films. Matisoo and other individual researchers tacked back and forth between those horizons.

In 1965, so little was known about either the fundamental or the practical aspects of Josephson junctions that Matisoo had to tack back and forth just to get started. What materials could such a device be made from? How quickly could it switch states? Could it be fast enough and manufacturable enough to be a competitive microelectronics technology? True, to answer these questions Matisoo had to do some basic research. But he also had to develop a hands-on familiarity with superconducting materials and with circuits made from those materials. That know-how might lead to an ultrafast computer or to a Nobel Prize. That is, Matisoo's research might zig or zag, but it wasn't yet on a linear path to anything.

For Matisoo to figure out whether the Josephson junction might eventually become a product or lead to a prize, he first had to play catch-up, especially with John Rowell at Bell Labs but also with groups at Ford Research, at Arthur D. Little, at GE, and at the University of Pennsylvania. At first, catching up with the field's state of the art meant simply learning to make single experimental Josephson junctions. (Devices that could do something useful were a long way off and might prove impossible.) On paper, the junctions seemed straightforward to fabricate: take a substrate (Matisoo tried gold, later silicon), evaporate a superconducting strip (early on, usually lead or tin) onto that substrate, grow a thin insulating layer (often an oxide of the superconductor) on top of the superconducting strip, and finally deposit another superconducting strip (often the same material as the first superconductor, but not always) on top of the insulating layer.

The biggest problem was forming the insulating barrier. Many materials easily grow an insulating oxide (think of rust growing on a piece of iron), but the best superconductors for making a Josephson device do not. Luckily for Matisoo, over the course of the 1960s the superconducting electronics community developed tricks for growing or depositing oxide (and other insulating) layers on a variety of superconductors. However, forming an insulating barrier is only half the battle; the other half is getting the barrier thin enough to allow tunneling but not so thin that it loses integrity and forms "pinholes" that short-circuit the device. Over the lifetime of IBM's Josephson computing efforts, problems with the insulating barrier would appear, get temporarily resolved, and then reappear as the Josephson team moved to more complex architectures or thinner barriers.

By 1966–67, as Wilhelm Anacker later recalled, Matisoo had advanced from fabricating single junctions to "demonstration of subnanosecond switching of Josephson tunneling devices, and in 1967 [to] the operation of a thin-film Josephson device flip-flop, both indicating that Josephson switching devices could indeed be switched very fast and could be competitive with projected semiconductor integrated circuits." "On the basis of these encouraging results," Anacker continued, "the pros and cons of Josephson devices were assessed and an initially small research program was launched in 1967 with the aim of studying technological and system aspects."[20] That is, Matisoo's discoveries opened up enough possibility of application that they led to an "initially small research program" supervised by Anacker and located in Yorktown's Applied Research department. At the same time, the Josephson junction's potential in basic research was evidently not exhausted, and therefore Matisoo, located in the Physical Science department, continued work focused on fundamental questions and scholarly publications. Within IBM, at least, the Josephson junction zigged *and* zagged simultaneously.

With the additional personnel and resources that Anacker could bring in, IBM pulled ahead of rival Josephson efforts. Other groups—whether corporate, academic, or government—now looked to IBM to see where the technology was headed. Some former leaders in corporate superconductivity research—GE, Arthur D. Little, Ford—ceded the field to IBM. Others, notably the Sperry Corporation and Bell Labs, reconfigured their superconductivity efforts as defensive hedge bets. About one-tenth the size of IBM's effort, Sperry and Bell Labs' Josephson teams existed in part to maintain awareness of and capacity in Josephson microelectronics so that those firms could rapidly catch up if IBM were to succeed.

Ambivalent Support from Inside, Enthusiastic Interest Outside

In the 1970s, any knowledgeable observer would have identified IBM as the firm that was betting the most on Josephson technology. Two kinds of knowledgeable observers were especially important to IBM's Josephson gambit: high-ranking researcher-administrators within IBM and federal grant officers (particularly in the National Security Agency). Ultimately, Josephson computing's patrons within IBM were supportive but not particularly enthusiastic. The NSA proved more optimistic but wasn't able to offer much financial backing under the straitened federal budgets of the 1970s and the early 1980s.

The difference between the IBM Josephson program's internal and external patrons largely boiled down to conflicting views about the feasibility and the necessity of scaling up chip manufacturing to very large volumes. In theory, the switching speed of a Josephson junction could be extraordinarily fast—much faster than anything that can be realized in silicon even today. Yet a complex chip built from large numbers of Josephson junctions would necessarily be considerably slower than the theoretical limit for a single junction. Moreover, the compromises that would have to be made in order to manufacture Josephson chips reliably, cheaply, and in profitable quantities would lead to further losses in speed. For the NSA (which was accustomed to buying small numbers of nearly handcrafted supercomputers, such as the Cray-1), mass manufacturability was a minor consideration relative to the outstanding theoretical limit of Josephson logic.[21] For IBM executives, used to selling vast numbers of computers to business customers, the practical limits to a manufacturable Josephson computer were always in mind.

Nevertheless, some of the leaders of IBM Research thought Josephson computing merited serious examination because their assessments of future trends in conventional semiconductor technology were dire. IBM Research put considerable effort into understanding the theoretical limits of semiconductor electronics in the 1960s and the 1970s, generating results that were widely celebrated across the physical and engineering sciences. Probably the most famous finding from this work was Rolf Landauer's discovery that "logical operations that get rid of information [as most operations in conventional computers do] … necessarily require the dissipation of energy."[22] Landauer's principle was taken as proof that Maxwell's demon cannot overturn the second law of thermodynamics—a major corrective to Victorian physics' greatest theorist. More practically, in modern microelectronics Landauer's principle places a limit on how small a logic element can be and still erase information.

Landauer, as much as anyone, created the environment in which Josephson computing rose *and* fell at IBM. As Charles Bennett and Alan Fowler put it in *Biographical Memoirs of the National Academy of Sciences*, he was "an outstanding scientific and technical manager of IBM's Watson Research Laboratory, guiding it from relative obscurity to become by 1970 one of the world's two most important and innovative engineering and scientific laboratories."[23] Under Landauer's tutelage, IBM Yorktown became an exemplary site of what Jonathan Zeitlin describes as firms' "strategic reflection and deliberate experimentation" regarding the "salience of alternative possibilities" for products and processes.[24] Josephson computing was merely one of several alternatives to conventional semiconductor microelectronics that Landauer and other senior IBM scientist-managers tracked and debated over the years.

Landauer was friendly with the Josephson group and initially open to the possibility of Josephson computing. As one of the participants in the Josephson program told me, "Rolf, [Laughs] I think he was a proponent. Not a very strong one but, you know, Rolf was skeptical about a lot of things."[25] As we will see, Landauer's skepticism increased and eventually came to be directed toward almost all unconventional microelectronics technologies. Much the same could be said of Landauer's friend and colleague Robert Keyes; indeed, Keyes and Landauer were sometimes talked about as though they were intellectual twins. As Landauer's obituarists put it, "he understood what was needed to build a computer very well and along with Robert Keyes tried to pass such knowledge to the promoters of every cockamamie scheme that emerged. As a result he took a dim view of optical computing, [and] logic based on threshold devices, such as Esaki diodes and Josephson junctions."[26]

Yet Landauer and Keyes' "dim view" of "cockamamie schemes" emerged only gradually, and co-evolved with IBM's Josephson program. During the period when that program's outcome was still not yet known, their attitudes were more open. One participant told me that Keyes "wasn't exactly against it and he wasn't exactly for it."[27] Though less famous in general scientific circles than Landauer, Keyes was an influential voice in the microelectronics industry in the early 1970s. He was particularly well known for two articles published in 1969 and 1975, "Physical Problems and Limits in Computer Logic" and "Physical Limits in Digital Electronics," in which he tried to put a lower bound on the size of components in an integrated circuit and an upper bound on the number of components that could be crammed into an area.[28] Keyes was awarded the Institute of Electrical and Electronics Engineers' W. R. G. Baker Award for the latter article in 1976, the same year he was elected to the National Academy of Engineering and became an editor of *Reviews of Modern Physics*. As those distinctions indicate, his views on semiconductor scaling and superconducting logic were influential both within and beyond IBM just at the time when the Josephson program was gaining momentum.

Historians of nanotechnology have identified Keyes' papers on limits to miniaturization as two out of about seven articles that cited Richard Feynman's "Plenty of Room at the Bottom" speech in the first twenty years after 1959, when it was given.[29] That speech has since come to be seen, rightly or wrongly, as the origin of nanotechnology—yet so few contemporaries ascribed any significance to it that Keyes alone accounted for more than one fourth of its early citations. As we will see in the next two chapters, several other early fans of Feynman's speech were, like Keyes, influential advocates of microelectronics-driven changes in American science in the 1970s.

Keyes' papers should also be seen as a manifestation of the "limits" discourse that was ubiquitous in the late 1960s and the early 1970s—a discourse that usually is associated with shortages of oil, energy, water, food, clean air, and other resources, but which also had an echo in some microelectronics experts' skepticism about Moore's Law.[30] In that vein, Keyes' articles were in dialogue with more optimistic articles by figures associated with the Intel Corporation, especially Carver Mead (a professor at Caltech and a consultant to Intel) and Robert Noyce (co-founder of Intel).[31] Though I don't want to reduce Moore's Law to an expression of alleged Silicon Valley techno-optimism, it is true that the Bay Area high-tech corridor in which Intel was located—and which it shaped—was becoming increasingly unfriendly to talk about limits to resources at the same time that Noyce and Mead were disputing Keyes' calculations of limits to the miniaturization of integrated circuits.[32] Noyce himself certainly took umbrage at the notions of resource scarcity being put forward by Garrett Hardin, Paul Ehrlich, and the Club of Rome in this period.[33]

Noyce's optimism about Moore's Law has become a self-fulfilling prophecy in part because Silicon Valley firms stuck to a conservative, incremental engineering philosophy. In contrast, large, established firms such as IBM and AT&T repeatedly explored more daring and disruptive engineering solutions to the limits problem—though usually they later returned to the conservative, incremental path taken by Intel. For instance, whereas Intel and its Silicon Valley peers stuck with optical lithography for carving small features in silicon, IBM and Bell Labs pushed for adoption of electron-beam and x-ray lithography and even more exotic fabrication techniques, including molecular-beam epitaxy.[34] I'll explain the technological differences behind these "lithography wars" in chapters 4 and 5; for present purposes it should suffice to note that established firms with a large basic research capacity were willing to pursue disruptive manufacturing processes *and* unconventional microelectronic products such as Josephson junctions. Conversely, Silicon Valley start-ups with limited in-house basic research capacity concentrated on incremental improvements to silicon transistors and optical lithography.

IBM explored many unconventional alternatives to silicon. Josephson computing was one; molecular electronics, the subject of chapter 3, was another. Compound semiconductors (semiconducting materials such as gallium arsenide that are made up of more than one element) have been another perennial alternative because, like Josephson junctions, their theoretical limits appear to be much faster than silicon's. Even more exotic alternatives—DNA computing, quantum computing, silicon on sapphire, optoelectronic interconnects, and others—have come into and gone out of favor. Each of these schemes looked more promising in Yorktown than in Silicon Valley because their theoretical capabilities looked favorable in light of IBM scientists' predictions that silicon would soon be unsuitable for maintaining Moore's Law. Conversely, Silicon Valley firms (and such fellow travelers as Carver Mead and James Meindl) didn't show much interest in Josephson computing or in other exotic microelectronics because they were more optimistic about the long-term potential for miniaturizing integrated circuit components in silicon.

Optimism about silicon almost always meant pessimism about Josephson microelectronics; pessimism about silicon usually meant an inclination to tentative optimism about Josephson microelectronics. That rule of thumb is a good guide to Robert Keyes' writings on limits to miniaturization. For Keyes, the crucial limiting factor in scaling down integrated circuits was the waste heat generated by tiny, closely packed transistors. As was discussed in chapter 1, the bipolar transistors on which IBM relied need some constant input of current and therefore dissipate more heat than the MOS transistors that Intel adopted. As the components of an integrated circuit get smaller and more closely crammed together, it becomes more and more difficult for them to dissipate heat. In both his 1969 article and his 1975 article, Keyes warned the industry that power dissipation was a rapidly approaching problem and pointed to various exotic technologies that might provide relief:

> The stunning success of silicon semiconductor technology for information processing has not completely stifled the search for alternative technological bases for memory and logic. … There has been interest in and research related to logic based on superconducting devices, fluid devices, magnetic bubbles, and even optical devices, in the past decade. Superconducting devices based on the Josephson tunneling cryotron appear to be the most likely candidate for logic that will make a much larger, faster computer possible; a Josephson gate that switches in only picoseconds and has a power dissipation of microwatts has been described.[35]

To Keyes and Landauer, the fact that Josephson junctions could switch ten to twenty times faster than silicon transistors suggested that it might soon be practicable to incorporate Josephson logic in a viable computer architecture. More important, the fact that Josephson junctions consume thousandths of the power needed for a bipolar transistor meant that they had much greater potential for miniaturization, and therefore *future* speed, than bipolar junction transistors. That is, Josephson logic's low power requirements made it seem possible that there could be a Moore's Law for superconductors that would replace Moore's Law for silicon.[36] For a company with IBM's resources, that possibility was too tempting not to explore, even if very few people inside IBM—and virtually no one in Silicon Valley—thought it would succeed.

Outside the industry, Josephson junctions looked more promising. In particular, some government agencies pursued superconducting circuits (and other alternative forms of microelectronics) because of the unique requirements of their missions. For instance, NASA and the military services needed circuits that could survive exposure to radiation better than conventional silicon transistors could. The National Security Agency was also interested in superconducting electronics because its unique signals-processing and cryptography applications put a premium on speed, not manufacturability. Thus, once Anacker's group formed in 1967, two NSA researchers, Fernand "Doc" Bedard and Nancy Welker, began tracking IBM's progress closely.[37]

By the summer of 1971, Bedard and Welker had reached the conclusion that Josephson junctions were promising enough that it would be worth trying to build a computer using them. To help design that computer they brought in John Pinkston, an electrical engineer with experience in specialized architectures for cryptographic and signals-processing applications—even though, Pinkston later recalled, he "didn't even know what superconductivity was at that point." Bedard, Welker, and Pinkston began driving up to Yorktown Heights regularly to discuss ways the NSA could aid IBM. In December of 1972, those discussions resulted in an agreement for the NSA to supply about $1 million per year to (in Pinkston's words) "augment" the IBM program, in return for which IBM would report to NSA on its progress toward benchmarks that the NSA and IBM had negotiated. The NSA contract also came with an expectation that IBM would incorporate certain features that met the NSA's specific needs—such as the ability to do digital-to-analog and analog-to-digital conversion—even if they weren't useful to most other customers.

As figure 2.1 indicates, the NSA contract coincided with a period of rapid innovation in IBM's Josephson technology. Over the course of 1973 and 1974, the rate at which IBM applied for patents on inventions related to Josephson computing more than doubled. In addition the geographic scope of the project broadened, as a team at the IBM laboratory near Zurich joined the Yorktown effort. Eventually a rough division of labor was arrived at, Zurich putting more emphasis on advanced materials and memory and Yorktown on logic and system architecture. With the addition of the Zurich team, the total number of personnel grew from about twelve to fifteen when the NSA contract was signed to more than thirty four years later.

Around 1977, Ralph Gomory, IBM's Director of Research, approved a significant expansion of the effort, with the aim of proving or disproving the commercial feasibility of a Josephson computer once and for all. New personnel flowed in, the number peaking at around 125 a few years later.[38] Many if not most of these people were, like John Pinkston at the NSA, relatively unfamiliar with superconductivity when they started, if only because the program grew so quickly that the global pool of superconductivity experts wasn't large enough for IBM to hire only people trained in superconductivity.

Extendibility

At its peak, IBM's Josephson program absorbed about $20 million per year, the NSA kicking in roughly a quarter of the budget.[39] That kind of money brought with it a degree of accountability that the program hadn't labored under in the past. The NSA required monthly progress reports, and Bedard, Welker, and Pinkston made quarterly trips to Yorktown. To demonstrate accountable progress to both NSA and the IBM management, the team began building a "cross-sectional model" (CSM) of a Josephson chip made up of Josephson logic elements, operating at low temperature, with the full complement of input and output electrodes, a "clock" driving each cycle of switching within all the logic elements, and a few instruments embedded in the chip to measure its performance. The CSM was "cross-sectional" in that it had fewer gates than a fully functional chip. It wasn't designed to do useful computations, much less sophisticated cryptography. Rather, it was a test of the unglamorous packaging, refrigeration, wiring, and other things that would have to be developed in house (since IBM was still largely vertically integrated) before a functional chip could be mass produced.

Once the Josephson project began focusing on manufacturing-related matters, IBM managers with expertise in mass production of *silicon* devices became more involved. That may seem counterintuitive, since the whole point of the project was to develop a computer not based on silicon transistors. Yet managers who knew how to manufacture silicon had experience that was surprisingly relevant to the Josephson program. For one thing, many of IBM's experts on manufacturing had already been through similar transitions—from vacuum tubes to discrete transistors to integrated circuits—and it was hoped that they would supply a steadying hand through one more transition.

Perhaps more pertinent, even though IBM's Josephson computer would not rely on silicon *logic*, it would rely on silicon the *material*, about which IBM's experts on manufacturing knew plenty. Juri Matisoo put it this way in a review article:

> A computer made up of Josephson junctions constitutes a radical departure from a well-established semiconductor technology. The fabrication of Josephson-junction components relies, however, almost entirely on methods learned in the development of semiconductor devices. The substrate material chosen for the Josephson-junction chips is silicon, not because of its conducting properties but because techniques for forming precise microscopic structures on silicon are well established. Circuit patterns are defined photolithographically, as they are in making semiconductor devices.[40]

In other words, to make their "radical" technology manufacturable, the Josephson team would have to persuade—and secure the expertise of—exactly those experts on conventional semiconductor manufacturing whose authority and accomplishments would be undermined by the success of Josephson computing. The fortunes of the Josephson team's members now depended on people who weren't particularly inclined to aid them.

I don't want to give the impression that the deck was insurmountably stacked against Josephson technology, or that IBM's manufacturing managers were "biased" in any crude way. As one of the proponents of Josephson computing told me, IBM's silicon experts "had their reservations, as did other people, even people who were working on [the Josephson project]. It wasn't clear, in spite of all the work we did, and all the bright people we had, that the cross-section would be a success."[41] Rather, I am claiming that most of the parties to IBM management's decisions regarding Josephson technology—both proponents and critics—operated under the belief that Josephson logic and silicon logic were competitors, but that the mass production of Josephson logic would depend upon techniques originally developed for the mass production of its silicon rival.

By 1980, IBM had invested enough in Josephson logic so that upper management wanted to know just how competitive with silicon it actually was. If it appeared that a commercial Josephson-based computer could in fact outsell a silicon-based rival, then huge sums could be justified to bring the technology to market through the sequential stages of the linear model. If not, then Josephson junctions could retreat into basic research labs at IBM and at universities, running the linear model in reverse. The task of formally making the comparison between Josephson logic and silicon logic fell to Emerson Pugh, a physicist and manager well suited to balancing the somewhat conflicting interests of IBM's research and manufacturing arms.[42] In 1980, Ralph Gomory, IBM's Director of Research, asked Pugh to assemble a team composed of both semiconductor and superconductor specialists to conduct an "extendibility study" of *both* Josephson technology and silicon bipolar technology. That is, Pugh's committee was expected to predict whether Moore's Law would "extend" miniaturization of silicon bipolar logic over the next fifteen years *and* whether an equivalent of Moore's Law would emerge for superconducting logic—and, if it were to emerge, how far and fast the miniaturization of Josephson technology would "extend" relative to silicon technology.

The results were ambiguous. Pugh's committee estimated that a Josephson chip might, someday, be between three and six times as fast as a bipolar chip with the same power dissipation.[43] However, the extendibility study also noted several potential obstacles to mass producing Josephson chips, one of which had to do with "punchthrough"—that is, logic elements' accidentally flipping their state as the clock cycled. They also found several areas in which bipolar technology would continue to outperform Josephson technology. Above all, bipolar technology was more "extendible" than Josephson technology—that is, with bipolar technology it would be easier to scale down the components and integrate them into complex chips. "Recently," the extendibility study reported, "new bipolar technologies have emerged … which promise greatly reduced power-delay products" and which would therefore allow bipolar transistors to be packed closer together without incurring the power dissipation problems that Robert Keyes had identified a decade earlier.[44] But the superior extendibility of bipolar technology was also attributed to a perceived problem with Josephson technology. In particular, a Josephson-based memory (based on trapping or expelling a single magnetic flux in a superconducting ring) didn't appear to be amenable to much scaling down. That is, Pugh's committee foresaw no Moore's Law for superconducting electronics.

Probation and Production

Critics soon seized on the extendibility study as evidence that the Josephson program should be shut down. One skeptic, Robert Gunther-Mohr, put it as follows in a memo to Ralph Gomory:

> I believe this has been [a] useful effort. It has increased mutual awareness between the Silicon and the Josephson efforts in IBM and has helped us understand both opportunities and limitations more comprehensively than before. The complexity of the problem has required considerable judgments in arriving at conclusions. I believe a factor of at most 3 between CIL/JSP [Josephson] Technology and Silicon, with all the technical uncertainties, gives an insufficient justification for [continuing with a strong commitment to the Josephson program].[45]

Josephson advocates, however, seem to have used the upper estimate of a sixfold differential in speed to persuade management to give them the resources they would need to achieve the target of "ship[ping] general-purpose, high-end Josephson machines for the commercial market" by 1990.[46]

Still, the extendibility study revealed enough uncertainties in Josephson technology that a price had to be paid for keeping the program going. In March of 1980, three months before his committee reported its findings, Emerson Pugh sat down with Wilhelm Anacker and outlined seven benchmarks for a ten-year course leading to commercialization of Josephson technology. The first was completion of the CSM; the second was that a "corporate task force will review project and make a decision on its future" within nine months. When that task force met at the end of the year, twelve of its fifteen members voted for cancellation![47]

One of the three who favored continuing was a prominent engineer and manager named Joseph Logue. Logue was an IBM Fellow (the company's highest honor), in part because of his contributions to IBM's transition from vacuum tubes to transistors in the 1950s and its transition from discrete transistors to integrated circuits in the 1960s. In 1980, however, Logue did not believe that the company was poised to make a similar transition from transistors to Josephson junctions. Instead, Logue voted to continue the Josephson effort because "the program had been so badly mismanaged that it was difficult to determine if the basic problem was mismanagement or technology," and that therefore he couldn't vote for cancellation until a more competent management had shown the technology's true potential.[48]

Now, that's not a view of Wilhelm Anacker's management abilities that I have heard affirmed by any of the veterans of the Josephson project I have interviewed. But, of course, most of Anacker's subordinates were fellow researchers, rather than experts on the design and manufacturing of commercial systems. Thus, Logue's evaluation should be seen largely as a symptom of a gulf between IBM's researchers and the people responsible for IBM's commercial systems. Anacker ran the Josephson program as a scientist, tolerating side projects on the basic physics of superconductivity, changes in design, and design and materials choices that made sense for a research project but weren't conducive to manufacturing—an approach that Logue would naturally have seen as "mismanagement," but one that made sense in the context of the corporate *research* laboratories at Yorktown and Zurich.

For example, under Anacker there was a multi-year disagreement between Yorktown and Zurich as to what material should be used for the Josephson junctions. Yorktown favored lead, Zurich niobium. Lead is easier to work with and therefore well suited to a one-off prototype or laboratory demonstration, but it is poorly suited for a commercial device. Lead Josephson junctions tend to degrade quickly because lead is so soft that any cycling in the temperature of the environment around it will cause it to deform. And yet, Anacker delayed making the decision to drop lead and adopt niobium until *after* the 1980 extendibility study, thirteen years into his supervision of the Josephson project.[49]

From Logue's perspective as someone who had shepherded commercial circuits and whole computer systems through the development process and onto the market, Anacker's tardiness in adopting niobium crystallized Logue's assessment of the Josephson project's mismanagement. From Anacker's perspective, though, lead junctions may have seemed necessary for so long because they facilitated the project's *success*. That is, variations in junction design could be made and tested more quickly with lead than with niobium, thereby allowing more ideas to be put forward and winnowed, leading to more rapid convergence on a better design. And the Josephson program did make enormous progress under Anacker—progress in proving the basic concept of a Josephson logic element, in developing fabrication techniques for making such devices, in modeling how an integrated Josephson logic would work, and in designing an architecture and packaging for such a system.

The metrics of success that guided Anacker's management of the program, though, were those that prevailed in IBM Research, rather than in the firm's manufacturing arm. The Josephson team included some members of the Advanced Materials and Technology group at IBM's manufacturing facility in East Fishkill, New York, but most participants were at the labs in Zurich and Yorktown. For Logue, that was probably *prima facie* evidence that the program's feasibility could not be adequately determined until an expert on the manufacturing of commercial systems took charge.

At any rate, Logue's intervention persuaded Ralph Gomory not to cancel the program outright. But Gomory also accepted Logue's criticism of the program's management. Wilhelm Anacker was summarily removed, and Juri Matisoo was promoted away to the Corporate Technical Committee at IBM's headquarters. According to Logue, Gomory tried to get several people to take Anacker's job before finally offering it to Logue himself. "I suspect I was the only one insane enough to take it," Logue later recalled. "I said that I would accept the offer, but he [Gomory] must understand that I had only one chance in 10 to be successful and that it would take two years to determine if the program would fly."[50]

In order to "fly," the Josephson program would have to develop processes for manufacturing chips and other components that would go into a commercially competitive system. To help the Yorktown research staff better understand manufacturing considerations, the team built a small fabrication line inside the Watson Research Laboratory—something very rare (perhaps unprecedented) at Yorktown before then but similar to R&D practices at Silicon Valley firms.[51] The Yorktown line made only small numbers of devices; its purpose was not to make chips but to help the researchers understand what problems might crop up as chips were made.

A true pilot line was built at East Fishkill, and some Yorktown researchers were transferred there to oversee the line's construction and operation. The administrative ambiguities of the Fishkill pilot line illustrate how wide the gulf was between research and manufacturing at IBM in this period—but also how the Josephson program helped narrow that gap. As Logue recounts it, "many of the 50 PhDs on the team were more interested in work that would lead to an individual publication than in working as a team to advance the program."[52] That may be a tendentious description, but it is true that many Yorktown PhDs preferred doing research (at Yorktown) to doing development (at Fishkill). Some members of the Yorktown staff even demanded that they be given official letters stating that they were only being temporarily seconded (rather than permanently transferred) to Fishkill and that they could later return to Yorktown.[53] And yet, some Yorktown researchers learned from their time at Fishkill that problems of manufacturing could present intellectual challenges that were as satisfying as basic research questions. Certainly, several young researchers who stuck through the final two years of the Josephson program went on to careers in which they merrily tacked back and forth between basic research and management of manufacturing.

Leveraging Failure

As Logue predicted, however, IBM's Josephson program didn't achieve its stated goal—a "general purpose, high-end Josephson machine for the commercial market," or even the more modest goal of "special purpose units … for a small number of customers" (presumably the NSA, other national-security agencies, and advanced users of scientific computing).[54] Joseph Logue had stipulated that the program had two years to succeed under his direction, so after those years expired in 1983 Emerson Pugh was again asked to conduct an extendibility study to determine the program's fate. This time, his committee concluded unambiguously that Josephson technology was a dead end. And so, on September 23, 1983, the program was canceled.

In the second study, the trend lines for Josephson technology were compared not only against silicon bipolar but also against gallium arsenide, another of the perennial contenders to unseat silicon. In other words, IBM management was still intensely interested in alternatives to silicon, but Josephson technology was no longer the "most likely candidate" that Robert Keyes had thought it was in 1975. Both Josephson technology and gallium arsenide chips performed even less favorably in comparison with silicon bipolar in 1983 than Josephson technology alone had just three years earlier. Commercialization had slipped to "the mid 1990s." Josephson memory seemed even less extendible (i.e., miniaturizable), and therefore less able to compete with silicon in the long term. Finally, it was now clear that resolving the "punchthrough" problem (and other "noise" issues) would require compromises that would slow a Josephson system down.[55] The most optimistic performance estimate for Josephson technology was now only *five* times as fast as silicon (and gallium arsenide only four times better), and even that level of performance was "not expected to be realized until early in the 21st century."[56]

Each of these considerations could, on its own, have persuaded IBM managers not to commercialize either Josephson technology or gallium arsenide. Thus, every partisan of Josephson technology I have asked has given a different reason why the program was canceled. I would speculate, though, that the most persuasive reason for cancellation in 1983 was silicon's spectacular recent success, rather than Josephson technology's ongoing problems. In 1980, when the first extendibility study was conducted, the global semiconductor industry was on the threshold of one of the deepest slumps of the postwar era, a slump that lasted through the Josephson program's probationary period. But in 1983–84, sales rebounded, leaving manufacturers scrambling to meet the demand. According to Intel and World Semiconductor Trade Statistics, the years 1980–1983 saw barely any growth in semiconductor sales, but 1984 saw the largest single-year increase in sales for any year between 1968 and 2004 (nearly three times the second-largest increase).[57]

In that environment, by 1983 the Josephson fabrication line at East Fishkill looked a double waste of money—superconducting logic wasn't likely ever to displace silicon on the market, and the Josephson pilot line had displaced silicon-manufacturing equipment that IBM now needed badly. As Alan Kleinsasser, one of the Yorktown PhDs seconded to Fishkill, later recalled, "the joke was always that 'There are bulldozers roaming the halls. As soon as they close that [Josephson fab line] that corridor's going to be converted to a [silicon] packaging line or something. We [the Josephson team] are just a thorn in their [the silicon fab's] side.' I'm sure there was a lot of truth to that."[58] Or, as the second extendibility study put it, "in contrast to bipolars, the greatest risk for Josephson devices is that they may never be used at all."[59] So long as no one was buying silicon chips either during the bust years 1980–1983, the risk that no one would buy Josephson chips was acceptable. But by late 1983, when it was clear that the next year would see a boom in sales, the downside of diverting money, personnel, and real estate from silicon to Josephson became overwhelming.

All told, IBM put well over $100 million into Josephson technology—with inflation, a little more than a quarter of a billion 2016 dollars.[60] What did it get for its money? Most obviously, it obtained certainty that Josephson technology was *not* worth pursuing. That might not seem worth the cost, but the value of that knowledge should be judged in the context of all the exotic alternatives to silicon floating around IBM and elsewhere for years. IBM had put its best effort into what was widely believed to be the most viable alternative to silicon, and it had failed. From the late 1960s to the early 1980s, article after article about the future of electronics predicted that Josephson logic "may initiate a new era of extremely rapid processing equipment" and identified IBM as the giant that would make that a reality.[61] If IBM couldn't get Josephson technology to work, then silicon really was here to stay. Rolf Landauer summarized this ruefully in 1990:

> There are many advanced technology proposals which become major thrusts, only to be abandoned again subsequently. An adventurous technological climate has to reward the taking of risk, and must allow failures. But there are too many cases where everyone eventually realizes that the flaws were understood at an early stage, but the dissident voices not heeded. … Among the many supposedly broadly applicable logic proposals we have seen come and go, we can find Gunn effect logic, tunnel diodes, ferrite core logic, schemes utilizing combinations of electroluminescent devices and photoconductors, fluid logic, parametric microwave excitation and Josephson junctions. Some technological candidates, such as Josephson junction logic, magnetic bubble storage, or the battery powered automobile, did deserve real examination. When they are discarded, it is done with trepidation, and knowledge that the decision may not last forever.[62]

Note how Landauer puts Josephson logic in a special category: a failure, but the only alternative to silicon logic that deserved "real examination." And as a special, particularly ambitious case, the examination of Josephson logic was more instructive for IBM managers' reflections on their own organization than other explorations of less plausible alternatives to silicon. The Josephson program called into question basic assumptions about the role of research at IBM. After all, if IBM engaged in "blue-sky" research in the hope that it might occasionally yield "game-changing" technologies, then there should be some means by which research discoveries could actually change "the game." The Josephson program, however, highlighted the structural impediments to translating research discoveries into manufactured products: Very smart researchers were not self-evidently equipped to manage large manufacturing operations, nor were managers with experience in manufacturing self-evidently committed to commercializing ideas that came from the research lab.

This is not to say that IBM *should* have gone ahead with the Josephson project. Superconducting logic does have a certain romance, but every member of the Josephson team I have spoken to has come around to the view that IBM was right to cancel the project (though they can't agree on *why* that decision was correct). Rather, my point is that there were many people who thought at the time that Josephson computing was the kind of thing that should move from research to manufacturing, but that the difficulties the Josephson team faced in achieving that transition made it clear that there were substantial obstacles to *any* piece of blue-sky research traveling from Yorktown and Zurich to East Fishkill. The alternative that IBM and other large firms gradually adopted in the 1980s was to retreat from blue-sky research and have their labs work on projects promising more certain returns.

Yet if the Josephson program highlighted the hazards of blue-sky research, it also showed that basic research could secure IBM's reputation for innovation and high technology. Most famously, the Josephson project indirectly spun off research that gained four different IBM scientists shares of the 1986 and 1987 Nobel Prizes in Physics. The first was to Heinrich Rohrer and Gerd Binnig for their invention of the scanning tunneling microscope (STM).[63] Through the 1980s and the 1990s, Binnig and other IBMers invented dozens of variants of the STM, some of which grabbed headlines in the *New York Times* and became icons of nanotechnology research.[64] The connection between the STM and the Josephson project is rarely mentioned today, but early on Rohrer occasionally acknowledged that one of the motivations for inventing the STM was to help the Josephson group avoid the "pinholes" in thin films that had been problematic ever since Juri Matisoo first began fabricating Josephson junctions.

The 1987 Nobel Prize in physics went to Alex Müller and Georg Bednorz for their discovery of a class of ceramics that becomes superconducting at a higher temperature than any previously known class of materials.[65] Here the connection to the Josephson program is superficially more obvious. It would be an overstatement, though, to suggest that the Josephson program *led* to either Nobel Prize. It is likely the STM would have eventually been invented and high-T_c superconductors discovered without the Josephson program. Still, both prizes owed something to Josephson computing. Müller, for instance, spent a year and a half at Yorktown, "where he started working in the field of superconductivity," in 1978–79, just when the Josephson project was heating up.[66] He spent at least some of that time conversing with Yorktown's Josephson contingent, and continued to exchange results with them after returning to Zurich.[67] Similarly, Binnig's PhD research concerned both superconductivity and tunneling, and he was likewise hired at IBM in 1978, just when the Josephson team was expanding most rapidly. None of the four laureates was formally a member of the Josephson project, but all of them benefited from conversations with members of the team and with other IBM superconductivity experts. Their careers show how IBM positioned itself (at least until the late 1980s) to benefit from projects such as the Josephson effort whether they "zigged" toward production or "zagged" toward fundamental discovery.

Nobel Prizes were not IBM's only unexpected payoffs from the Josephson program. According to a retrospective account, "near the end of the Josephson computer program there was a realization by the IBM management and technical workers that good science could be done with technology developed by the Josephson computer team."[68] For instance, the fabrication line would occasionally make Josephson junctions for other IBM researchers working on superconductivity. Similarly, Mark Ketchen—later the designer of circuits for the cross-sectional module—invented a SQUID susceptometer that has now been an industry standard for thirty years.[69] Working on that device was more or less a sideline of the larger Josephson program, but such fruitful sidelines were fully in keeping with the way things worked at IBM Research in the late 1970s and the early 1980s. Here we see, again, the zig-zag—rather than linear—nature of innovation at IBM in that era. Solving a technological problem might lead to an intellectual puzzle just as much as vice versa.

Ketchen is also a prime example of the zig-zag nature of the later careers of some members of the Josephson team, and it is those people who probably represent the biggest payoff of the project to IBM and to US microelectronics R&D more generally. Ketchen himself shuttled back and forth between silicon and superconducting projects (and between research and development) throughout his career. Other veterans of the Josephson program played important roles in leading IBM from bipolar to MOS transistors in the early 1990s (and thereby helping save the company from bankruptcy), in directing corporate strategy, and in inventing important software algorithms.[70]

Juri Matisoo's career probably best exemplifies the regard IBM held for Josephson program veterans despite their failure to overthrow silicon. After his stint at corporate headquarters, Matisoo became director of the Almaden Research Center in San Jose, overseeing the construction of a new facility in the hills overlooking Silicon Valley. During Matisoo's time as director, the Almaden lab saw major achievements in commercially successful technology transfer. Perhaps most notable was the transformation of giant magnetoresistance (a phenomenon that had recently been discovered simultaneously at German and French labs) into a ubiquitous and profitable technology for storing data.[71] Then, in the late 1990s, Matisoo became Vice President of Technology Programs at the Semiconductor Industry Association. There, he helped construct the International Technology Roadmap for Semiconductors, the primary mechanism of the industry-wide coordination that was needed to maintain Moore's Law.

Beyond Big Blue

One way or another, then, IBM got some return on its investment in Josephson logic. But those same benefits—personnel, fundamental discoveries, prestigious prizes, better knowledge of what direction microelectronics technology was headed—could not accrue to IBM's partner, the National Security Agency. Nor were IBM's reasons for abandoning Josephson computing well regarded within wider national-security circles. Thus, in reaction to IBM's decision, the Office of Naval Research asked the Naval Studies Board of the National Research Council to assemble a panel to review the government's options for gaining access to the superconducting electronics believed to be necessary for national security.[72] The Navy was interested in superconducting electronics primarily for the purpose of long-range detection of "magnetic anomalies" (i.e., Soviet submarines). It is possible, though, that the Navy's instigation of the review was a cover for the NSA, since the NSA's sponsorship of IBM's Josephson program was nominally kept secret even from many of the program's participants. The Navy panel must certainly have been formed with the agreement of the NSA, since that agency was represented on the review board.

The review concluded that it was "definitely in the best interests of the Navy and the nation to maintain a vigorous effort in research and development in superconductive electronics."[73] In light of IBM's withdrawal, the panel recommended that the government build a fabrication facility to provide superconducting chips to government, academic, and industrial researchers seeking to move the technology forward. As we will see in chapter 4, that kind of government-sponsored shared-equipment facility for microelectronics research was beginning to take root at a number of universities in the early 1980s. In fact, one of the members of the review panel was Robert Buhrman, a member of Cornell's faculty who held various leadership positions at that university's National Research and Resource Facility for Submicron Structures, the flagship shared-equipment facility for microelectronics research funded by the National Science Foundation.

Yet the Naval Studies Board's vision of a national facility for the fabrication of superconducting electronics never came about. In part that was surely a matter of money: The facility was estimated to cost $2 million to build and between $1 million and $1.5 million annually to run. As *Electronics Week* noted at the time, "such a facility would require hard-to-get cross-agency support."[74] As we'll see in later chapters, though, cross-agency support *was* available for more conventional microfabrication facilities. It is hard not to conclude that a major reason why the superconductivity facility wasn't built was that cancellation of the IBM program drained interest in the technology to the point that more conventional academic microfabrication facilities such as Cornell's could meet the low level of demand for superconducting chips while also providing equipment and services to the much larger number of researchers working on semiconducting electronics.

Still, it is instructive to note what the Naval Studies Board thought might happen if a dedicated national superconducting electronics facility were not built:

> It is the opinion of the Task Group that, without this disciplined facility, the circuit technology momentum generated by the IBM project will be lost in the United States to the detriment of Navy and national interests. The Task Group also believes that any delay in funding will lead to a loss of researchers from the field, a reduction of confidence of other organizations supporting research and development in superconductive electronics, a diminution of the value of the $15 million already spent by ONR for this work, and an exacerbation of the lead already held by the Japanese in large superconductive circuits.[75]

Notice how the possibility of economic competition from Japan was conflated with threats to national security and how an institutional innovation (a public-private facility for microfabrication of superconducting electronics) was presented as answering both threats at once. It is impossible to understand the changes wrought in US science in the past forty years without noticing the ripple effects of the panicked fear that Japanese firms would eclipse their American counterparts in microelectronics in the same way they had in steelmaking, shipbuilding, and auto manufacturing.

In the particular case of superconducting electronics, the basis for that panic was a large program initiated by MITI (Japan's Ministry of International Trade and Industry), with some help from other government agencies, especially the Electrotechnical Laboratory (ETL) and the Ministry of Education. The MITI Josephson program was apparently coordinated with parallel efforts in silicon and gallium arsenide; the annual budget of the Josephson portion is estimated to have ranged from $100 million to $150 million from 1981 to 1989.[76] In the eyes of Americans acquainted with both programs, MITI's seemed like an uninterrupted extension of IBM's efforts: "Commented one observer, not entirely humorously, in bringing Josephson junction technology up to its present high state, 'IBM may have opened Pandora's box, and out jumped the Japanese.'"[77] Thus, it isn't particularly surprising that MITI's program came to roughly the same end as IBM's. In 1989, having demonstrated "the first prototype superconducting computer to execute man-made computer programs installed in the Josephson

ROM (read-only memory) chip with a Josephson central processing unit and RAM (random-access memory) chips," the program was canceled.[78] The social fact that the microelectronics industry is a *semiconductor* industry—not a superconductor industry—held true then and has held true ever since.

From the American side, perhaps the best way to understand the IBM Josephson program is as part of the transition from the early Cold War to the post–Cold War era. In the early Cold War, the government was *the* dominant customer for high-end microelectronics. IBM and other firms made huge profits building vast systems on the government's dollar, and then made even more money by adapting those systems to civilian markets. But by the 1970s, civilian markets were wagging the government dog, which was increasingly too cash-strapped to create its own markets. As John Linvill explained in a memo to the Stanford administration in 1978,

> the fact that the military establishment represents less than 10% of the business being conducted by the semiconductor industry in the United States concurrently means that the semiconductor industry will not automatically pursue military objectives in the face of a rapidly growing and powerful commercial market which is easier to deal with.[79]

Thus, NSA's support of the IBM program was paltry in comparison with what IBM itself put in. When IBM judged that Josephson technology would not be profitable on the civilian market, the NSA had no chance of saving it.

The rationale for the NSA's patronage had been the government's need to solve a "market failure"—that is, to keep alive some technology that was in the national interest but which no firm could develop on its own in the competitive environment of the microelectronics marketplace. Similar rationales are often offered for government sponsorship of high-tech firms. And yet, as Josh Whitford and Andrew Schrank have shown, federal grant officers may be more efficient in resolving "network failures"—situations in which it is in the national interest that two or more organizations cooperate but the actors don't trust one another and/or don't know enough about one another's competencies.[80]

In some countries at some times, the distinction between resolving market failures and network failures is moot. The MITI program, for instance, involved coordination of a large number of corporate and government laboratories, and even some academic ones, in order to share the burden of exploring a high-risk, high-reward technology. In that case, resolving market failures (by putting in cash) brought new partners into the network, and resolving network failures (by getting participants to know and trust one another) lowered the cost of bringing the technology to market. In the United States, however, market and network failures were usually more clearly delineated, at least rhetorically. Certainly, in the case of the IBM Josephson program, the mono-organizational nature of the effort is a hallmark of an attempt to address a market failure: The NSA wanted a superconducting computer, so it tried to pay IBM enough money to make up for the losses IBM would incur in developing an unprofitable product.

Tried, but failed. The costs of developing any alternative electronics, in the face of silicon's constant improvement and vast profitability, are just too much for any firm or state agency, and probably even for a coalition of firms and agencies, to bear. As we will see in the next few chapters, US government agencies have had more success in facilitating networked relationships among advocates of alternative microelectronics technologies than in trying to summon markets for particular microelectronics technologies into existence. The Josephson experience played only a minor role in that turn from a market-failure policy model to a network-failure paradigm. Yet the Josephson story is emblematic of why the market-failure paradigm is difficult to sustain, particularly when the network-failure paradigm has become much more attractive and more routine. Indeed, even though the NSA's support of the Josephson program was predicated on overcoming a market failure, it probably had its greatest effect in indirectly addressing network failures—failures of trust and knowledge—within different branches of IBM.

None of the examples of government support for microelectronics that we will encounter in coming chapters would count as an attempt to resolve a market failure; all of them should be seen primarily as attempts to ameliorate network failures. One certainly could find government interventions in the microelectronics industry along market failure lines in the late Cold War, but as the Soviet threat diminished the network-failure paradigm came to the fore. The many variations on that paradigm will be our focus from here on.

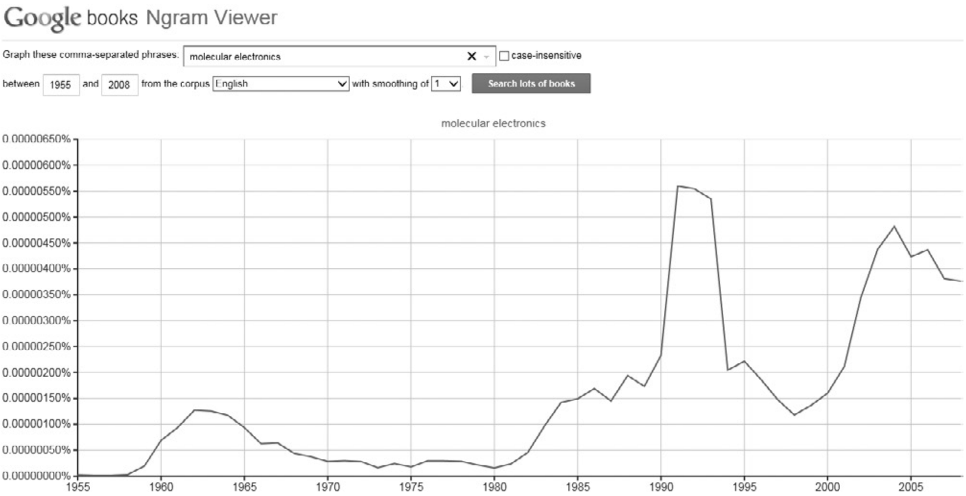
The network-failure paradigm doesn't always work, as the next chapter will show. But one of its advantages is that it complements the zig-zag model of innovation that researchers actually practice. The market-failure paradigm assumes the presence of a linear model of innovation in which a technology moves toward realization if enough money is poured in. And perhaps that is why the linear model was so influential in the period when the national-security state had the money to make markets rather than just host networks. But as IBM and other firms moved away from doing everything (including research) in house, and as the federal government was forced to use its money more efficiently, research came to be located much more in heterogeneous networks of actors than in “stove-pipe” units within a single organization. And in that environment, the zig-zag model of innovation is paramount, since the topology of the network is omnidirectional rather than unidirectional—actors, ideas, materials, and money are free to move in any direction through the network, going with equal probability from applied to basic or from basic to applied, and with equal probability from corporate to academic or from academic to corporate.

3 Molecular Electronics Gains a Reputation

As was explained at the beginning of the preceding chapter, conventional microelectronic devices are composed of tiny solid structures made from insulating, conducting, and semi-conducting materials. In the late 1960s, though, physicists and chemists became interested in building circuits from "organic conductors," a class of carbon-based materials (much like wool or cotton) that nevertheless conduct electricity with relatively little resistance (much like copper and gold). Today, organic conductors and a related class of materials, conducting polymers, are entering the market in a few technological applications, particularly organic light-emitting diodes (OLEDs). In the early 1970s, however, a few visionaries began proposing these materials as the basis for something more revolutionary: an "ultimate" form of microelectronics that would bypass Moore's Law. Co-opting the name of an earlier attempted revolution in semiconductor circuits, these visionaries called their dream "molecular electronics."

The multiple recurrence of molecular electronics should be readily apparent from figure 3.1, a Google Ngram of that phrase since its first appearance in the late 1950s. Each generation since has re-appropriated that phrase, always taking a different family of molecules as its referent. After a quick look at the first generation, this chapter will focus on molecular electronics' second avatar, which flourished from the early 1970s to the early 1990s. In chapter 6 we will look at molecular electronics' post–Cold War third generation.

The timing of molecular electronics' three generations makes it an excellent probe of the trends we have examined thus far: the effect of the Japanese government's 1975 announcement of its VLSI program on American science; the merging of national-security rhetoric and economic-competitiveness rhetoric among American policy makers; and the turn from addressing market failures to addressing network failures in federal intervention in microelectronics R&D. In addition, the strange career of



Graph these comma-separated phrases: molecular electronics ☐ case-insensitive

between 1955 and 2008 from the corpus English with smoothing of 1

Figure 3.1

A smoothed version of a Google Ngram for “molecular electronics.” This chapter surveys the first hillock (corresponding to the Westinghouse–Air Force program), but focuses on the second (the Aviram/Ratner/Carter version). The third hillock will be examined in chapter 6. Source: Google Books Ngram Viewer, http://books.google.com/ngrams.

molecular electronics allows us to look at the role of charismatic prophets in steering research organizations toward techno-utopian visions. In the United States—much more than elsewhere—molecular electronics has attracted a colorful cast of characters promising that an “ultimate” electronics would transform the world.

Breakthrough!!!

The lure of molecular electronics arises from the perpetual prospective crisis that has gripped the microelectronics industry since the mid 1950s or even longer.[1] That crisis has had many names over the years. For instance, as we saw in the preceding chapter, scalability or miniaturization “limits” were shorthand descriptors for the industry’s perpetual crisis in the 1970s and the 1980s. In the early 2000s, the same crisis was often referred to as the “red brick wall,” a gesture to the matrix of targets for miniaturization set by the International Technology Roadmap for Semiconductors that allow the industry to maintain Moore’s Law. In the ITRS’ matrices, objectives that can’t be met with any currently existing technology are colored red, so that most matrices feature a solid wall of red at the right side of the page. Targets colored red are an enormous incentive to firms to build connections with external researchers who might yield insights that will change the target to yellow (meaning that it is not currently achievable but the means to achieve it are known). Conversely, red targets are an incentive to university and government researchers to form connections with semiconductor firms on the chance that their ideas might help keep Moore's Law going.

In the 1950s, the impending crisis driving much of the innovation in microelectronics was referred to as the "tyranny of numbers."[2] The tyrannical "numbers" were counts of components (resistors, inductors, capacitors, transistors, etc.) in circuits, especially high-performance circuits for military customers. As circuits for radar, computers, and missile guidance systems grew more complex every year, the number of components in those circuits also grew. But the number of connections between those components grew even faster, since each component was connected to at least two others. Components were connected by soldered wires, and a failure in any of the soldered connections would cripple the circuit as a whole. The "tyranny of numbers," then, was shorthand for saying that as circuits grew more complex, their average lifetimes before failure became shorter.

The "tyranny of numbers" holds a special place in the history of microelectronics because the integrated circuit—still the basis of most advanced commercial electronic technologies—was invented as a way to overcome it.[3] The essence of an "integrated" circuit is that there are no soldered connections; the circuit is integrated monolithically into a crystalline substrate. Think of a complex circuit made from discrete components as like a house made from Lego blocks, whereas an integrated circuit is like the same house carved out of a mountainside. It should be obvious that the latter is more impervious than the former to ordinary physical contacts and shocks.

The role of the "tyranny of numbers" in the invention of integrated circuits is relatively well known. Less well known is the fact that the IC was only one of several solutions to that problem that were proposed at the time. All of the US military services supported programs to overcome the "tyranny of numbers", and those programs yielded alternatives to the integrated circuit. Both the Army's Micro-Module program and the Navy's Tinkertoy program involved integrating pre-fabricated libraries of subcircuits into a functional whole.[4] The Air Force's alternative, molecular electronics, employed long crystalline "ribbons" that somewhat resembled a vertically stacked (rather than planar) integrated circuit. These alternatives have been largely forgotten, eclipsed by integrated circuitry. At the time, however, they competed with, and in some cases merged with, integrated circuits. In fact, one of the most commercially important computers of all time—IBM's System/360, first sold in 1965—used a hybrid chip architecture, known as Solid Logic Technology, in which various sub-circuits were baked together, rather like Micro-Module, but the sub-circuits themselves were monolithic integrated circuits.[5] In other words, it was not self-evident in the early 1960s that integrated circuits would come to dominate other ways of making complex circuits. For more than ten years, ICs were one of several circuit types that competed with and complemented one another.

Curiously, while the integrated circuit was gradually overtaking other circuit types, it was also being dismissed as an outmoded, defunct technology that would itself soon be overtaken. Though the integrated circuit was invented as a solution to one crisis (the "tyranny of numbers"), the IC itself has been in crisis ever since. There have always already been proponents of alternative technologies who have warned that the integrated circuit—particularly the silicon integrated circuit—was nearing the limits of speed, power consumption, or miniaturization. As a result of those warnings, as we saw in chapter 2, billions of dollars have been spent to explore alternatives to what David Brock has called the "ecosystem" of silicon and optical photolithography.[6] Most of silicon's competitors—such as Josephson logic—have themselves been fashioned as integrated circuits. But a few alternatives have been put forward as successors not just to silicon but to the integrated circuit itself. What, though, could go beyond integrated circuitry? One answer, expressed in different ways over the decades, has been a circuit that is not carved from a single piece of material but is instead itself a single molecule—a "molecular" electronics.

The historian Hyungsub Choi has traced the origins of this idea, and of the "molecular electronics" label, back to the MIT laboratory of Arthur von Hippel in the mid 1950s.[7] For von Hippel, "molecular engineering" meant "build[ing] materials from their atoms and molecules for the purpose at hand" and "play[ing] chess with elementary particles according to prescribed rules until new engineering solutions become apparent."[8] As Ed Regis has pointed out, von Hippel's rhetoric—as expressed in articles and summer courses as early as 1956, and in the 1959 book *Molecular Science and Molecular Engineering*—was one of the unacknowledged inspirations for Richard Feynman's 1959 after-dinner speech "There's Plenty of Room at the Bottom," which has come to be seen as the founding moment for nanotechnology.[9]

More tangibly, von Hippel's ideas were taken up by Westinghouse research managers in the late 1950s as they attempted to attract military funding to help their firm catch up to the semiconductor industry's state of the art. Westinghouse had been one of the pioneers of the electrical industry in the late nineteenth century, and in the postwar period it continued to innovate in electricity-producing technologies such as nuclear reactors. In solid-state microelectronics, however, it was a minor player in comparison with RCA, GE, and IBM. Yet Westinghouse research managers saw in von Hippel's "molecular engineering" an evocative label for their efforts to move ahead of the established players.

Luckily for Westinghouse, just as the firm approached the Air Force for funding for a program in "Molecular System Engineering," the Soviet Union launched Sputnik and threw the American national-security state into panic. Suddenly funding was available for even rather unlikely technologies so long as their proponents promised to put the United States on a more competitive footing relative to the Soviet Union. Within months of Sputnik's launch, the Air Force—the youngest and most aggressively technophilic military service—identified Westinghouse's re-labeled "molecular electronics" as its chosen path to the nearly science-fictional technologies that would allow it to control air and space. As figure 3.2 indicates, Air Force R&D officers (particularly at the Electronics Technology Laboratory at Wright-Patterson Air Force Base) saw molecular electronics as the breakthrough that was needed to get past the limits imposed by integrated circuitry—limits they believed would be reached by the early 1960s.

But despite all the optimism, "molecular electronics" was still just a label. Neither the Air Force nor the term's champions at Westinghouse had any idea what it meant or how it would differ from integrated circuitry. It was only after several meetings and conferences that Westinghouse proposed an idea for drawing long, thin single crystals of germanium out of a melt and doping different regions of the crystal with atoms of other elements.[10] If this plan worked, the crystal *as a whole* would have the functionality of a transistor, a resistor, a capacitor, or perhaps even a more complex assemblage of such components. Since a single crystal is arguably a molecule, the circuit would draw on the material's "molecular electronic" properties.

The Air Force paid Westinghouse a little more than $7 million between 1959 and 1962 to develop this "dendritic approach."[11] Early optimism, however, soon gave way to grumbling that Westinghouse was delinquent in delivering a working technology. That pressure, in turn, forced Westinghouse to drop long-term development of dendritic germanium and instead try to catch up with the silicon IC technology that most other microelectronics firms had adopted. Even though the Air Force still had a "Molecular Electronics Branch" as late as November of 1965, and Westinghouse had a "Molecular Electronics Division" until 1968, it is clear that from 1962 on

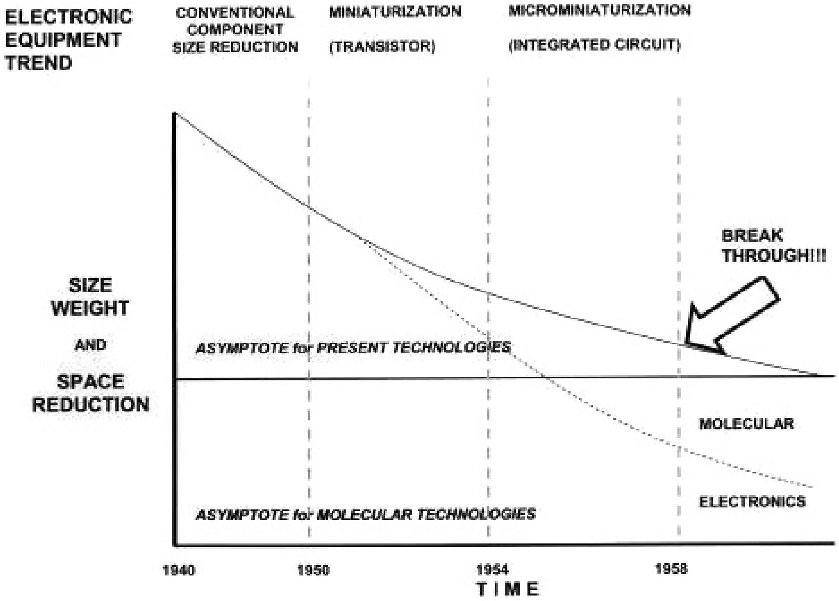


Figure 3.2

A chart of past and future miniaturization of conventional microelectronics technologies (vacuum tubes, transistors, and integrated circuits) and of the improved miniaturization that would result from switching to the Air Force's and Westinghouse's concept of molecular electronics. This chart was presented by the director of electronics at the Air Force's Air Research and Development Command. Source: C. H. Lewis, "The Needs of the Air Force," in Proceedings of NSIA-ARDC Conference on Molecular Electronics, Washington, November 13–14, 1958. Thanks to Hyungsub Choi for finding this graph. Further information can be found in Choi, Manufacturing Knowledge in Transit: Technical Practice, Organizational Change, and the Rise of the Semiconductor Industry in the United States and Japan, 1948–1960, PhD dissertation, Johns Hopkins University, 2007.

both organizations used the term as an idiosyncratic label for a technology that was virtually indistinguishable from mainstream silicon integrated circuits.[12] Before long, "molecular electronics" disappeared, and a term that initially signified radical innovation and creative destruction was itself eclipsed by the wider industry's incremental but rapid progress in silicon integrated circuits. And that, more or less, has been the story of "molecular electronics" every time the term has resurfaced as an alternative to silicon microelectronics.

Materials and Bandwagons

Before we get to the next iteration of molecular electronics, we should take a short detour into organic chemistry. That's because molecular electronics' reappearance in the 1970s made use of innovative materials and institutions that emerged a few years earlier at the intersection of organic chemistry and applied physics. As we saw in chapter 2, in the postwar period the economic importance of the microelectronics industry coincided with the growth of the United States' research capacity in a way that promoted the study of the electronic properties of unusual materials. Physicists, chemists, materials scientists, electrical engineers, and others studied exotic materials *both* in hopes of making fundamental discoveries and in hopes of advancing the technological state of the art in microelectronics. Occasionally research of this sort would identify new classes of materials with unusual electronic characteristics that would rapidly attract large numbers of investigators. High-temperature superconductors were one example in the mid 1980s; carbon nanotubes, graphene, and other carbon allotropes were among the others.

In the late 1960s, a research field of this type began to coalesce around "organic conductors"—materials composed of molecules containing carbon that nevertheless have a low electrical resistance more characteristic of metals such as gold and copper. Indeed, an alternate name for organic conductors and similar materials is "synthetic metals." As exotic as that sounds, a few organic conductors are quite common and have long histories. Graphite, for instance, contains *only* carbon and is a fairly good conductor. In general, however, these materials attracted little interest until the late 1960s, and it is likely that the field would have grown more slowly had it not been for the excitement and controversy generated by a startling discovery that later had to be "walked back."

The non-discovery that sparked interest in organic conductors concerned a charge-transfer (CT) salt known as tetrathiofulvalene-tetracyanoquinodimethane, more commonly referred to as TTF-TCNQ. TTF molecules readily donate an electron, and TCNQ molecules readily accept electrons. The compound TTF-TCNQ consists of stacks in which TTF and TCNQ alternate, so that the overlapping electron orbitals of the acceptor and the donor allow electrons to move relatively freely up the stack. Thus, each stack has a "conduction band" usually seen only in metals or semiconductors and not carbon compounds—though with the difference that in TTF-TCNQ electrons conduct somewhat freely along the stacks but are much more constrained in moving in other directions. TTF-TCNQ's conductivity is said to be "anisotropic," whereas in metals conductivity is "isotropic" (that is, more or less the same in all directions).

In 1968, a young physics professor at the University of Pennsylvania, Alan Heeger, was preparing to take a sabbatical at the University of Geneva when Tony Garito, a graduate student in another professor's group, came to him to discuss the possibility of using conducting organic salts—especially compounds of TCNQ—to explore a theoretical construct known as a "one-dimensional metal" (that is, a material with anisotropic conductivity such that it behaved like a metal along one dimension but not along the other two).[13] Heeger was just then winding down research on magnetic impurities and looking for new topics, so he told Garito that he could join Heeger's group as a postdoc once Heeger returned from Geneva. Around the same time, a few other groups also became interested in organic charge-transfer salts. In 1972, when one such group at Johns Hopkins University reported synthesizing TTF-TCNQ, Heeger and Garito eagerly adopted the new material. Within months they published data that they interpreted as showing that the conductivity of TTF-TCNQ rose dramatically at around 58 degrees Kelvin.[14] Though their sample's conductivity decreased again at lower temperatures, Heeger and Garito indicated that they believed they had found a "superconductivity fluctuation," a precursor to full-blown superconductivity. If correct, that would make TTF-TCNQ not just an extremely rare organic superconductor but also a—for the time—very-high-temperature superconductor. This was big news indeed.

Unfortunately, it was not news that survived other groups' attempts to reproduce their results. Eventually, Heeger and Garito had to pull back from their claim of a superconducting fluctuation. As Heeger recalled 2005,

> This two year period was … a really hard time for me because this got to be such a controversy. … Fortunately we were able to bring out the basic physics, but there were many fights and many name-callings. I remember we had a conference at Lake Arrowhead [California]. It was the end of the conference, and I was sitting in the front row. It had been a tough day and there had been a lot of contention. Aaron Bloch, a professor at Johns Hopkins, got up and made some cynical comment about our work. I got so angry I literally attacked him! I started chasing him around the table, but he's a very tall guy and had long legs so I never caught him. … [Another time] I went to Bell Labs to give a talk. … I knew it was going to be a tough day. And, just imagine doing this today—I had a toy gun in my briefcase. The first thing I did was opened up my bag and put the gun down on the table![15]

In the end, Heeger and Garito acknowledged that, although the dramatic rise in conductivity that they observed was real, it was not a superconducting fluctuation; most likely it was caused by a small crack in their sample.

Although they lost that particular battle, they made great progress toward winning the war. Scientific interest in TTF-TCNQ accelerated rapidly after their initial announcement of a superconductivity fluctuation.

Even as the TTF-TCNQ controversy was unfolding, Heeger was searching for other "synthetic metals" to work with. Soon he came across an article indicating that an inorganic polymer, polysulfur nitride, might have similarly unusual metallic properties. Heeger began looking for a chemist who could help him synthesize and understand this material. Before long he was referred to Alan MacDiarmid, a professor in the University of Pennsylvania's chemistry department. MacDiarmid had worked with sulfur nitride ("SN") as a master's candidate, so he was well prepared to synthesize its polymer "$(SN)_x$." Reportedly, though, his first meeting with Heeger started off on the wrong foot because he thought Heeger was saying "Sn_x" and "wasn't particularly impressed that tin is a metal"![16] Nevertheless, Heeger and MacDiarmid struck up a collaboration that would last more than a decade. Notably, their collaboration was, in part, a product of the same post-Sputnik panic that drove the Air Force to sponsor Westinghouse's molecular electronics program.

Both Heeger and MacDiarmid were affiliated with the University of Pennsylvania's Laboratory for Research on the Structure of Matter, one of the first three Interdisciplinary Laboratories (IDLs) funded by the newly formed Advanced Projects Research Agency in the wake of Sputnik. (The other two were at Cornell and Northwestern.) ARPA's IDL program fostered forms of interdisciplinary collaboration that were unusual at the time but that have come to be the norm in American academic science and engineering.[17] MacDiarmid and Heeger's partnership was one of the first fruits of that institutional promotion of interdisciplinarity.

Like the Westinghouse–Air Force version of molecular electronics, the IDLs were founded to overcome a perceived crisis in the development of technologies related to US national security. In the IDL's case, that crisis was known as the "materials bottleneck," a shorthand for the widespread belief that important weapons systems were being delayed by a lack of advanced materials.[18] Thus, the IDLs were founded in the early 1960s to throw physical and engineering scientists together in the hope that doing so would yield new materials for weapons and aerospace vehicles. Hyungsub Choi and Brit Shields have argued, though, that at first ARPA believed that the IDLs' contribution to resolving the materials bottleneck would be to train PhDs who would then go to work for military-industrial firms. In the late 1960s, however, ARPA began to insist that the IDLs *themselves* generate mission-critical materials—and indeed Heeger and MacDiarmid did just that with conducting polymers.

Materials for microelectronics were within the IDLs' purview, and a few of the IDLs, including those at Cornell and Stanford, specialized in them. As we will see in coming chapters, the IDLs were immensely important in the reconfiguration of university-industry-government partnerships around microelectronics starting in the late 1970s. Several of the universities that led the way in experimenting with new institutions for microelectronics research did so by building on a foundation provided by their local IDL. This was particularly true of MIT, Cornell, and Stanford. At each of those schools, and in American science more generally, the IDLs provided an early template for interdisciplinary, industry-oriented research that was applied repeatedly starting in the late 1970s.

In Heeger and MacDiarmid's case, Penn's IDL provided a vehicle by which the chemist and the physicist could become aware of, and trust, each other's capabilities. In that sense, the IDLs were an early instance of government intervention to ameliorate network failures within the academy. The next step in their partnership also required state intervention to facilitate networking among academic researchers. In 1975, MacDiarmid took a sabbatical at Kyoto University, and while in Japan he was invited to give a lecture at the Tokyo Institute of Technology. Afterward, he was introduced to a junior faculty member, Hideki Shirakawa, who showed him a sample of the material he was working on: polyacetylene.[19] Noticing its silvery color, MacDiarmid realized that polyacetylene might have properties similar to those of polysulfur nitride. On the spot, MacDiarmid asked Shirakawa if he would be willing to relocate to Penn for a year if MacDiarmid could obtain funding for a postdoctoral fellowship. Shirakawa agreed, and so when MacDiarmid returned to the United States he got in touch with Kenneth Wynne, the Office of Naval Research grant officer who was funding MacDiarmid's work on polysulfur nitride, to ask for extra money for Shirakawa's visit. As MacDiarmid recounted in 2005,

> Ken naturally asked me why, and I told him, "Well, you know, I've never seen this silvery polymer before." He laughed and asked, "Do you expect me to give you a new grant because it's something that's silvery? That's ridiculous … but why don't you write me a letter about it anyway?" So I wrote him a two page letter. And I thought that would be the end of that. … Later I got a phone call from him and he says, "Alan, I think you're crazy and I'm crazy, but why don't you write a formal proposal for twenty-one thousand dollars? You know nothing about organic chemistry, you know nothing about polymers. And you're asking me to fund something because of its color." I told him yes, that was basically it.[20]

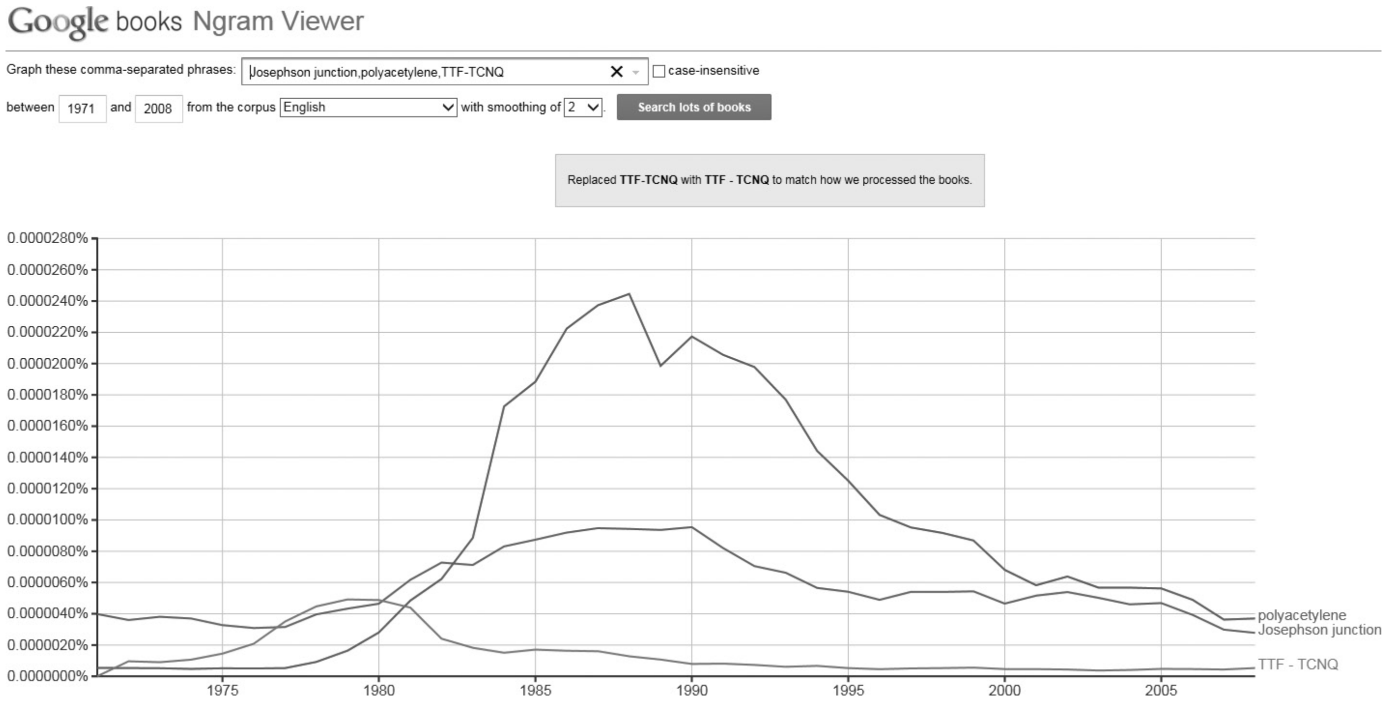
But Wynne's bet on MacDiarmid's "crazy" proposal paid off. During Shirakawa's brief stay in Philadelphia, he and members of the Heeger and MacDiarmid groups discovered that doping polyacetylene with bromine, iodine, and other impurities could raise the polymer's conductivity eleven orders of magnitude or more.[21] As Heeger noted later, with some understatement, "that experiment got the field of conducting polymers started and it was very exciting."[22]

Many of those who had been working on TTF-TCNQ and other nonpolymeric organic conductors dropped what they were doing and took up polyacetylene and its variants. Many more researchers who knew something about polymers dropped what *they* were doing and also took up polyacetylene. Conductive polymers were one of the most important "bandwagon materials" of the late 1970s through the 1980s. As a retrospective acknowledgment of their role in igniting widespread interest in synthetic metals, Heeger, MacDiarmid, and Shirakawa were awarded the Nobel Prize in chemistry in 2000. Ngrams of word frequencies (figure 3.3) show the mini-boom in "TTF-TCNQ" followed by a much larger and more rapid growth in interest in "polyacetylene." For comparison, figure 3.3 also includes the count for "Josephson junction"; by this rough estimate, the conducting polymers community would have been about twice as large as the superconducting electronics community by the late 1980s.

Ultimate Miniaturization

The next iteration of the term "molecular electronics" was closely tied to research on organic conductors and conductive polymers. To understand how molecular electronics evolved out of the discoveries at the University of Pennsylvania, it is important to examine the multivalent attraction of organic conductors and conductive polymers. On the one hand, these materials were amenable to investigations of fundamental physics and chemistry and the testing of theoretical constructs such as one-dimensional metals. On the other hand, it was easy to imagine technological applications for organic conductors, and even more so for conductive polymers. A conductive material with the cheapness and manufacturability of a plastic might be useful in all kinds of devices. One review put it this way in 1984:

> In contrast to the CT salts [such as TTF-TCNQ], the widespread fascination with conducting polymers is clearly motivated by the technological potential. However, as in the case of CT salts, these materials have also presented a strong scientific challenge. This challenge has attracted a diverse community of chemists, physicists, and materials scientists who have not previously been active in



Graph these comma-separated phrases: Josephson junction,polyacetylene,TTF-TCNQ ☐ case-insensitive

between 1971 and 2008 from the corpus English with smoothing of 2.

Replaced **TTF-TCNQ** with **TTF - TCNQ** to match how we processed the books.

Figure 3.3
A Google Ngram for “TTF-TCNQ,” “polyacetylene,” and “Josephson junction.” Source: Google Books Ngram Viewer (http://books.google.com/ngrams).

> polymer science. The resulting involvements and contributions have gone a long way toward contradicting the view of some scientists that “there is no science in polymers.”[23]

That is, organic conductors and especially conductive polymers lent themselves well to the zig-zag model of innovation that permeated many American corporate research laboratories. The results of experiments with these materials could easily, if unpredictably, zig or zag toward either prestigious basic research or product-relevant applied research. As we saw in chapter 2, superconductivity held much the same attraction for corporate researchers. In fact, one IBM research manager who was instrumental in starting that firm's Josephson computing program, Philip Seiden, also provided the stimulus for transforming organic conductor research into the new molecular electronics. The motivation for that evolution was an interest on the part of Seiden and Bruce Scott, an IBM Yorktown chemist, in improving IBM's share of the printer and photocopier markets at the expense of the market leader, Xerox.

Organic photoconductors (organic materials that change their conductivity when struck by light or other electromagnetic radiation) seemed promising candidates because they could be used to electrostatically transfer a pattern from a printed page without infringing on Xerox's patents. Organic conductors also seemed likely to be more manufacturable than the amorphous selenium coatings that Xerox was using. For these reasons, IBM's laboratory in San Jose had begun research on organic conductors even before the first synthesis of TTF-TCNQ at Johns Hopkins, and Seiden and Scott pushed Yorktown into the game just a little later.[24] Scott also seems to have hoped, early on, to develop an electron-beam lithographic resist from TTF-TCNQ, and in 1980 members of his group filed a patent for such a resist.[25]

Among those drawn into the Yorktown work on organic conductors was a young chemist name Ari Aviram. At the time, Aviram had a master's degree and reportedly harbored a sense that advancement at Yorktown would be made easier if he had a PhD.[26] In addition, his work with TTF-TCNQ and his background in chemistry had led him to think about how electrons would behave in a single molecule of this substance rather than in the bulk compound. As he put it later,

> I was trying to find some relevance between chemistry [his discipline] and the needs of the IBM corporation, which is basically a computing company. … I saw an analogy between some properties of molecules and properties of electronic components, and tried to determine whether these molecular properties could be utilized in the future in an electronic computer.[27]

Before long, Aviram seized on the possibly transistor-like properties of single molecules of organic conductors as a topic for a dissertation leading to his long-sought PhD.

Surveying chemistry departments near Yorktown for a potential adviser, Aviram identified a theorist at New York University, Mark Ratner, who had worked on electron propagation in molecules. As Ratner tells it,

> One day a guy walks into my office unannounced. He knocked on my door and said, “My name is Ari Aviram. And I would like to get a PhD in Chemistry.” I told him, “Well okay, but I don’t direct the chemistry department. There’s someone else you need to talk to.” And he said, “No, no, I want to do a theoretical thesis. I know there are two other theorists, but you’re the one I want to work with, because I know what you’ve done in the past. … And not only that, I think I can get *you* a gig giving lectures at IBM.” And I thought “Wow, this is awesome!”[28]

Before long, Ratner was going up to Yorktown regularly, giving lectures, and developing collaborations with Aviram, Scott, and Seiden:

> I learned a lot from Ari and his colleagues at IBM. They were pretty smart about circuits, and electronics, and all the stuff I didn’t know. And I knew something about molecular structure, the electronic structure of molecules, and even something about how a molecule’s electrons can talk to each other.[29]

With Scott, Ratner explored the characteristics of Magnus’ green salt, a semi-conducting polymer with the same property of anisotropic conductivity as the charge-transfer salts.[30] With Aviram and Seiden, however, Ratner turned his attention to an even more exotic idea of more lasting significance: the “molecular rectifier.”

A rectifier is a device that turns alternating current into direct current. Simple rectifiers use diodes to do this; since a diode allows current to move freely in one direction but not at all (or very little) in the opposite direction, in a rectifier the diode “clips” the alternating current, leaving only a direct current in the desired direction. Aviram’s idea was to design a single diode-like molecule that would have the functionality of a rectifier. In retrospect, it is easy to see how he got to that point. A diode, after all, usually consists of a piece of semiconductor divided into two regions: one doped to donate electrons, and one doped to accept them. IBM made lots of diodes of this type; more important, the bipolar junction transistors that IBM relied on at the time resembled, roughly, two such diodes connected back to back. In his organic conductor work, Aviram was becoming familiar with TTF-TCNQ, which at the molecular level consists of electron-donating and electron-accepting regions. Thus, the environment around Aviram contained ingredients that, when put together, led him to ask whether one

TTF molecule and one TCNQ molecule might, if assembled, work like a diode.

That question makes sense in retrospect, but at the time it was highly unorthodox. So Aviram offered three lines of justification for this work which have formed the creed of molecular electronics research ever since. First, he looked back to the history of electronics—in much the same way Westinghouse and the Air Force had—as a succession of distinctive technological platforms, with molecular electronics as the next in the series. As an IBM news release put it,

> Thus far, the components which carry out the processing of electrical energy have moved through three "generations": (1) the vacuum-tube and its other large-scale assistants, such as paper capacitors and the like; (2) the transistor, solid-state diode, and other "discrete" solid-state devices; and (3) integrated circuits which at increasing levels of miniaturization combine a host of various electronic devices. … [Aviram and Ratner] have proposed the design of individual molecules which would be able to act as functioning electronic devices in circuitry. … If they can successfully demonstrate this principle experimentally, then it is possible we may enter a new era, one of "organic electronics," in which electronic devices will flow from the chemist's laboratory in much the same fashion as synthetic fabrics do today.[31]

Simply being a next "generation" electronic technology wasn't enough, though. After all, the Westinghouse–Air Force version of molecular electronics was put forward as the next generation technology to succeed integrated circuitry, yet it had failed.

Aviram's second justification was that his molecular rectifier could miniaturize circuitry far more than conventional integrated circuits. Again, the similarity to the narrative represented by figure 3.2 is striking—the history of electronics might be one of dramatic, epochal breaks, but the miniaturization trend is captured by a smooth curve that crosses platforms without disruption while heading toward some asymptotic limit. As Aviram put it in the opening sentences of his dissertation,

> There is a trend in modern electronics toward miniaturization of circuitry and devices which meets with new successes continuously. One may wonder whether there are theoretical limits as to the smallness of a component. Taking a clue from nature, that utilizes molecules for the carrying out of many physical phenomena, it may be possible to miniaturize electronics components down to molecular size.[32]

The "trend in modern electronics" that Aviram referred to was, of course, Moore's Law. But notice how his reference to "theoretical limits as to the smallness of a component" upends Moore's prediction. For Moore, and for Intel and most of the other Silicon Valley firms, past reductions in the sizes of microelectronic components provided inductive evidence that such reductions would continue for about ten years—the period of time that Moore and his successors thought of as the foreseeable future. "Theoretical limits" didn't figure in Moore's Law because they wouldn't be reached within that time horizon.

But Aviram wasn't at Intel. He was at IBM, where his gesture to theoretical limits was entirely at home. Recall from chapter 2 that in the early 1970s IBM made an intense, well-publicized effort to determine the ultimate limits to miniaturization of solid-state components. IBM's forecasters were pessimistic that silicon transistors could be miniaturized enough to extend Moore's Law much further, and therefore encouraged explorations of alternative forms of electronics such as Josephson computing. If someone comparable to Robert Keyes or Rolf Landauer could get behind Aviram's "organic electronics" and "molecular rectifiers," the idea would have some staying power.

But Keyes and Landauer were never particularly optimistic about alternatives to silicon either. Over the course of the 1980s they became increasingly and vocally skeptical of such schemes generally, and of molecular electronics in particular. Thus, Aviram developed a third justification for "organic electronics" meant to combat such skepticism—an "organic" existence proof:

> Electronic components on such small scale are (in a sense) not original: Nature developed comparable miniaturization several billion years ago in living organisms—though the mechanisms of, say, nerve-impulse transmission and perhaps even memory are quite different in detail.[33]

This justification from biology was the most distinctive rhetorical difference between the first and second generations of molecular electronics, and it has become a permanent fixture of the field. Today's molecular electronics researchers happily collaborate with, or hail from, the life sciences. But in the 1970s, before the tides of American science had turned so strongly in favor of biomedicine, using biology to justify a new approach to making integrated circuits was quixotic. Aviram's gesture to biology would become conventional in the 1990s, but until then it appealed more to those at the margins of American science than to those in its mainstream, thereby contributing to molecular electronics' reputation as a fringe idea.

That reputation might have been avoided if Aviram and Ratner had shown experimental progress toward realizing their idea. As it turned out,

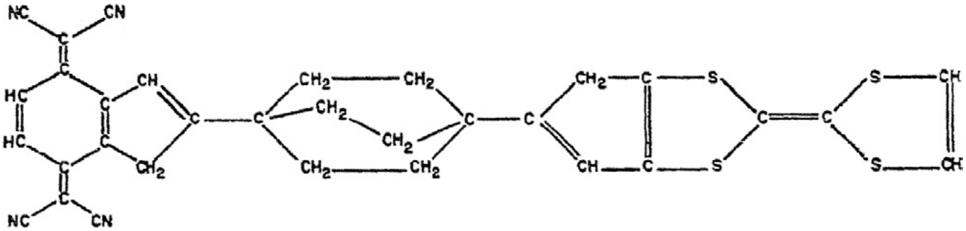


Figure 3.4

The Aviram-Ratner molecular rectifier, with an electron-donating TTF on the right, an electron-accepting TCNQ on the left, and a gating group in between. Reprinted from Arieh Aviram and Mark Ratner, "Molecular Rectifiers," *Chemical Physics Letters* 29.2 (1974): 277–283, copyright 1974. Used with permission from Elsevier.

though, the molecular rectifier would remain experimentally elusive for more than twenty years. Aviram and Ratner did publish an article in 1974 presenting a "design" (figure 3.4) for a molecule with a TTF group at one end, TCNQ at the other, and a sort of gate in between to ensure that electrons would localize at one end or the other rather than smearing over the molecule as a whole.[34] *Time* presented a short summary of their idea, and Aviram and Seiden took out patents on the rectifier molecule and an "organic memory device" based on a thin-film array of similar molecules.[35] But after that, the molecular rectifier almost vanished. As Ratner put it later, when their 1974 "paper came out, nobody read it, of course. It disappeared like a ghost."[36] In part, that disappearance was a product of the dissolution of Ratner and Aviram's collaboration. In 1975, Ratner moved to Northwestern University and gradually lost touch with his former student. IBM, meanwhile, in Ratner's words, "exiled him [Aviram] to printer ink"—most of Aviram's patents from the late 1970s and the 1980s are broadly related to copier and printer technology.[37] As we'll see, Aviram was not finished with "organic electronics"; as far as most of the outside world could tell, however, IBM had given up on the idea.

Turning Conducting Polymers into Molecular Electronic Devices

The apparent disappearance of Aviram, Ratner, and IBM is surprising from the vantage point of the early twenty-first century because the Aviram-Ratner "Molecular Rectifiers" article is now generally regarded as the origin of modern molecular electronics. Yet Aviram and Ratner themselves didn't use the phrase "molecular electronics" at the time and weren't able to stoke widespread interest in their paper until the very late 1980s. In the meantime, the revitalization of the “molecular electronics” label and the emergence of an American research community dedicated to molecular devices like Aviram’s and Ratner’s fell instead to Forrest Carter, a charismatic network builder at the Naval Research Laboratory.

Like Aviram, Carter came to molecular electronics as a result of the boom in research on organic conductors. Recall that Ken Wynne of the Office of Naval Research had been the principal grant officer supporting Alan MacDiarmid’s work on polysulfur nitride. Wynne had also supplied the funds for Hideki Shirakawa’s stay at the University of Pennsylvania, during which Shirakawa and members of MacDiarmid’s and Alan Heeger’s lab groups discovered that doping could raise the conductivity of polyacetylene many orders of magnitude. Thus, the ONR was deeply invested, literally, in the conducting polymer field in the mid 1970s. The Navy also had particular technological demands for which conducting polymers seemed to hold promise—for instance, a material that could conduct electricity without corroding in a shipboard environment exposed to saltwater would be tremendously useful. So in the late 1970s, Wynne began working with Fred Saalfeld, superintendent of the Naval Research Lab’s Chemistry Division, to coordinate an intramural NRL Electroactive Polymers (EP) program with Wynne’s portfolio of extramural grants for organic conductor research.

Carter was drafted into Saalfeld’s program, probably because he was an expert in x-ray photoelectron spectroscopy and in Auger electron spectroscopy—two of the analytic techniques that would be needed to characterize conducting polymers and their corresponding monomers. As the reports from the EP program’s annual symposium show, Carter was a formidable presence in that program—in the 1979 report, for instance, he authored or co-authored almost a third of the papers. At the same time, though, Carter was positioning himself to move from electroactive polymers into something more speculative and visionary.[38] And here—to be speculative myself—it is useful to inspect Carter’s biography and reports of his personality to try to figure out the roots of that vision.

Two features of Carter’s career before he joined the NRL are suggestive. First, he was a PhD student at Caltech in the late 1950s, where he worked in part with Linus Pauling and reportedly partied with Richard Feynman.[39] Though his friendship with Feynman should perhaps be treated with some skepticism, it is certainly true that Carter was a member of the tiny cadre of people who cited Feynman’s 1959 “Plenty of Room at the Bottom” speech before 1990.[40] Carter also possessed an expansive charm that resembled Feynman’s. Like Feynman, he had a taste for exotic hobbies (hot rods,

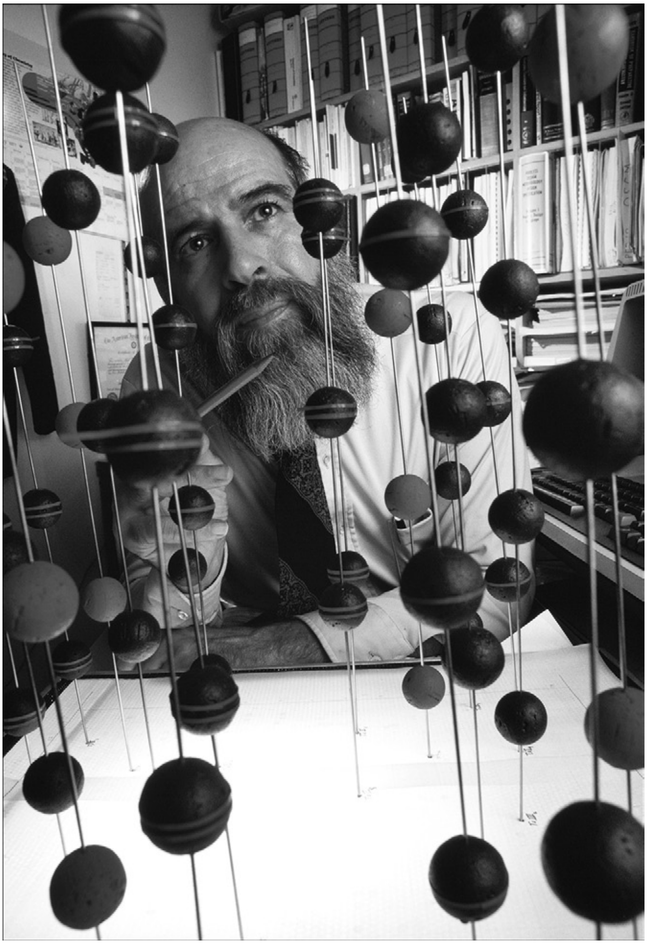


Figure 3.5
Forrest Carter, as photographed for *National Geographic* in 1982. From Allen A. Boraiko, “The Chip: Electronic Mini-Marvel That Is Changing Your Life,” *National Geographic* 162.4 (1982): 421–458. Credit: Charles O’Rear/National Geographic Creative.

motorcycles, fencing, platform diving, salsa dancing). And, like Feynman, he became known for extraordinary parties, risqué banter, and a coterie of young acolytes.[41] Carter’s striking appearance (see figure 3.5; almost every article about him mentions his “flowing grey beard”), rumbling voice, and dramatic presentation style (cited by critics and supporters alike) personalized molecular electronics as neither Aviram nor the engineers at Westinghouse and the Air Force had.[42]

The second suggestive detail of Carter's earlier career is that after Caltech but before moving to the NRL he had worked at Westinghouse in exactly the period during which the first generation of molecular electronics rose and fell. Although he was based in Pittsburgh rather than in the molecular electronics group (located near Baltimore), and although he seems not to have been directly involved in the effort, he almost certainly would have been aware of it. Indeed, his research at Westinghouse was related, at least in part, to semiconducting materials (though not germanium or silicon).[43] No definitive connection between Carter's Westinghouse years and the reappearance of "molecular electronics" can be made, but the fact that Carter had been exposed to that term in the 1960s before becoming its most visible proponent in the 1980s is striking.

Carter's Westinghouse experience is especially suggestive because, of course, nothing in the NRL's Electroactive Polymers program necessitated talk of "molecular electronics." Carter's use of that term set his writings for the EP program's reports significantly apart from those of his NRL colleagues. Whereas those colleagues used the reports to describe their incremental progress in doping bulk polyacetylene and polysulfur nitride and characterizing those materials' electrical properties, Carter chose instead to forecast the "Problems and Prospects of Future Electroactive Polymers and 'Molecular' Electronic Devices." In other words, he had disembarked from the quotidian world of conducting polymers and was headed toward something more futuristic.

Indeed, Carter was aiming at a much more ambitious and far-off target than even Aviram and Ratner. Whereas the IBM/NYU team had been content to imagine a single molecule that could act as a rectifier (one of the simplest circuit functions imaginable), Carter was now describing

> a "chemical" computer … based on components (diodes, gates, etc.) with molecular dimensions … constructed along a three- rather than the current two-dimensional architecture. Such a computer could provide a quantum jump advance, in terms of both speed and switch element density (e.g. 10^{15} per cm^3), over both existing systems and those planned as extensions of current semiconductor practice.[44]

No one had yet synthesized anything resembling the Aviram-Ratner rectifier. Even if someone had done so, synthesis would have been the easy part. After synthesis, one would have had to electrically "address" the molecule to see if it actually behaved like a diode—something no one had any idea how to do. Then, in order to realize Carter's "chemical computer," one would have had to synthesize quadrillions of molecules corresponding to different circuit components and somehow get them all to join together in the proper place and order to form a complex circuit. Not just the "chemical computer" as a whole, but also every step along the way to that computer was completely impossible with the technology of the time.

Impossible, but compelling. Carter often spoke of "molecular electronic device technology … as a way to leapfrog the current VLSI semiconductor based approach"—that is, a way to bypass Moore's Law and immediately reach the ultimate limits of miniaturization.[45] Such talk held obvious appeal for some of Carter's supervisors in a Department of Defense which was increasingly anxious that Japan's VLSI program would allow a foreign power to hop onto Moore's curve and keep pace with or outrun American firms in the race to produce smaller transistors. Especially within the US military, that eventuality was seen as a threat to both American national security and American economic competitiveness.

Thus, the US military—especially under the technophilic and free-spending Reagan administration—was desperate for alternative technologies that would allegedly keep the United States' microelectronics industry ahead of international competitors. Josephson computing was one possibility. Molecular electronics was another. Alan Berman, the NRL's director of research, put it this way in 1981:

> Are there requirements which could usefully employ computers that perform at rates 10^{12}, 10^{13}, 10^{14} per second[?] The answer comes out yes, indeed. … If I have the vision of such a need some 10, 20 or 30 [years] in the future, what are the technologies I ought to be investing in? At the moment, I see three possible directions to go. Two of these are … optical computing … [and] three-dimensional semiconductor devices. … In the [third] area of Molecular Devices, which I think may eventually be the winner in the competition for the race to 10^{12} operations per second sweepstakes, much remains to be done. One needs in the next 10 years a tremendous investment in this field.[46]

That "tremendous investment" never materialized, but Carter was able to get limited support from his supervisors for positioning "molecular electronic devices" as a quasi-successor to the Electroactive Polymers program. Within the NRL, molecular electronics work was essentially limited to Carter and an occasional postdoc. But externally, Carter and the NRL soon became the center of a dense and rapidly growing network of fellow travelers, and the focus of substantial, if ambivalent, publicity.

The event that nucleated this network was a workshop on Molecular Electronic Devices (MED) that Carter organized, with support from the NRL, in March of 1981. The lists of attendees and speakers make plain the multiple connections between Carter's version of molecular electronics and the American conducting polymers community that the Navy had fostered. Heeger and MacDiarmid contributed a paper, Ken Wynne served on the organizing committee and chaired a session, and the conference was funded by both the NRL and the ONR. Materially, the molecular "wires" Carter was proposing at this point were modified chains of polysulfur nitride and polyacetylene—the canonical conducting polymers of the time. Moreover, the theoretical contribution Carter developed in trying to understand information flow through a molecular circuit was the concept of "soliton switching"—an idea adapted directly from work on conducting polymers by Heeger and others.[47] Solitons are domain walls between different regions of a polymer that can move along the polymer like a particle. They don't carry charge, but Carter believed they could be used to carry information by switching a particular area of a molecular circuit from one domain type to another.

Carter's invitations to the MED workshops also indicate that he was thinking about an issue that Aviram and Ratner hadn't really confronted (though, no doubt, they also understood it to be a problem): how to connect a molecular electronic component to a more conventional integrated circuit, or at least to the metal wires through which any microelectronic circuit must communicate with the macroscopic world of power sources, input–output devices, sensors, and so on.[48] Several of the MED attendees came from the world of solid-state microfabrication and sub-micron lithography—for instance, one of the attendees at the first workshop was Michael Isaacson, a specialist in transmission electron-beam lithography and microscopy at Cornell's National Research and Resource Facility for Submicron Structures.

Carter seems to have hoped that the microfabrication community would supply techniques for making small inorganic electrodes to which organic components could be connected. Carter was ahead of his time, in that molecular electronics in the 1990s would advance in large part through collaborations between organic chemists and microfabrication specialists of exactly the sort Carter tried to foster. Indeed, perhaps the most visible microfabrication expert to enter the molecular electronics field in the 1990s—Mark Reed from Yale—attended Carter's third MED conference in 1986. At the time, however, microfabrication specialists weren't thinking in terms of molecular components and had little reason to move in that direction. Carter had to convince such people that their expertise in *micro*-electronics was relevant to *molecular* electronics. As Reed put it later,

> when I was at TI [Texas Instruments, prior to moving to Yale] I was doing all this [microfabricated] quantum dot work. I went to a bunch of conferences and one of them was held in DC by a guy by the name of Forrest Carter. … I remember getting the invitation, something about molecular structures, or maybe using molecular electronics. I thought "I don't know anything about that." But he said "no, we want you to talk about the fact that you can make these small devices." And I thought "sure, I can talk about that."[49]

A Louche Reputation

Carter's MED conferences did, however, draw a number of people who needed no convincing because they were already wildly enthusiastic about the future potential of molecular electronics, even if they were *not* particularly interested in more mundane prerequisites such as microfabricating quantum dots with optical lithography or characterizing the electrical properties of bulk polyacetylene at various dopant concentrations. Looking back in 1988, Hank Wohltjen—one of Carter's closest allies at the NRL—told the science journalist Mort La Brecque that "although [Carter's] original intent had been to invite a few key researchers from around the world, interest in the [first MED] meeting [in 1981] was so overwhelming that it soon grew to be a full-fledged conference." La Brecque quotes Wohltjen as having said "It got out of control in the early stages, in that some of the people there were proposing things that were very, very radical and had a flimsy scientific basis."[50]

Eventually, the "flimsy scientific basis" of some of the presentations at the MED meetings would get Carter into trouble. In the short term, though, the first MED conference catapulted Carter into the leadership of a research community that coalesced almost instantly. Carter had put a label on an idea that many people were already starting to become interested in, and he had created a forum in which those people could begin to network, share knowledge, and provide legitimacy for one another and for the general concept of molecular electronics.

Indeed, many of Carter's peers believed that his institution-building efforts were a much more important contribution than his work on the theory of molecular computing. As the chemist Noel Hush put it in a comprehensive review in 2005, Carter

> poured out ideas for molecular wires … [and] switches of many types. … More ambitiously, single-molecule logic-gates were proposed. … How feasible in practice this would be is problematic: its importance lies in the conceptual framework that was introduced and the interest that it aroused. Several conferences and two

> [*sic*: three] books edited by Forrest Carter based on these ideas appeared. … These aroused great interest and much discussion at the time about the feasibility of molecular-scale computing.[51]

Carter himself might or might not have acknowledged the infeasibility of his soliton switches, but La Brecque quotes Wohltjen as having said that Carter "would have agreed that much of his early work was highly speculative," but that "he felt that one of his main missions was to stimulate people to start thinking about the molecular-size domains as one that could perform useful functions.'"[52]

For providing that "stimulation," Carter was widely recognized in the 1980s and the early 1990s by the molecular electronics community that he helped create. Felix Hong, a professor of physiology at Wayne State University and one of the other leading molecular electronics institution builders in the United States in the 1980s, put it this way in 1989:

> The emergence of the field of molecular electronics is largely the consequence of one person's crusade, that of Forrest L. Carter. … He was a pioneer of molecular electronics and often appeared to be a lone prophet before his time. In recent years, he succeeded in gathering an ever increasing number of followers.[53]

A "lone prophet" Carter may have been, but a busy one, in view of the growing interest in molecular electronics. His research productivity—at least as measured in publications—had not been very high before the 1980s, but in that decade he churned out articles at a rapid pace for a wide array of edited volumes and special journal issues related to molecular electronics.

The first MED conference also secured Carter and the NRL favorable mentions in articles in the popular press about the up-and-coming technology of molecular computers. Particularly important in this regard was Kathleen McAuliffe's article "Biochip Revolution," which appeared in *Omni* just a few months after the first MED conference. Among other things, McAuliffe's article coined the word "biochip," a term that was soon being repeated in popular outlets and in what Sarah Kaplan and Joanna Radin have dubbed the "para-scientific" media: science magazines, such as *Chemical & Engineering News*, *Scientific American*, and *Physics Today*, that report science news both for public and professional audiences.[54] Such articles gained Carter, Aviram, and other molecular electronics enthusiasts reputations as "free thinkers" of biology-inspired computing.[55]

As the aforementioned articles indicate, part of what made Carter's proposals catch on in a way that Aviram's and Ratner's had not was popular awareness of, and growing adulation for, recombinant DNA techniques and the biotechnology industry. As we have seen, in the early 1970s Aviram gestured to biology as an existence proof for molecular devices; but at that time the modern biotechnology industry didn't yet exist. By the late 1970s, however, and especially after Genentech's record-setting initial public offering in 1980, biotechnology rallied hopes that the United States would use new high-tech industries to recover its economic competitiveness.

Thus, Carter, unlike Aviram, could associate "molecular electronics" with "molecular biology" to play on the latter's perceived potential to rejuvenate the American economy. Molecular electronics, by this telling, would merge the fecundity of biology with the precision of semiconductor manufacturing. As Jeannette Colyvas has shown, recombinant DNA techniques were frequently portrayed in the early days of biotech as a means for turning bacteria into "factories" for mass producing value-added goods from raw materials.[56] From there, it was a small rhetorical step for Carter and his new allies to portray viruses and bacteria as miniature computers. In fact, one of the first major profiles of Carter and the Aviram-Ratner-Seiden team was a 1982 *New York Times* article titled "Computers from Bacteria."[57] The same article also highlighted a biotech start-up, EMV Associates (also prominently featured in McAuliffe's *Omni* piece), which was "applying genetic engineering to the design of microcircuits."

Many of Carter's allies, then, were the types of people who read or were portrayed in McAuliffe's "Biochip Revolution" article in *Omni* and/or who were caught up in the fast-spreading faith that high technology would cure America's economic and cultural malaise of the late 1970s. As Patrick McCray has argued, a large proportion of *Omni's* readership were "cornucopianists" who viscerally reacted against early-1970s talk of limits to growth by seeking out visions of technologically aided limitless expansion not just of the economy but also of human lifespan, cognition, and bodily capability.[58] In the 1970s, many young cornucopianists expressed these desires through enthusiasm for the "space colony" concepts put forward by the Princeton University physicist Gerard O'Neill. But by the early 1980s the success and publicity of "genetic engineering" led some devotees of space colonies to embrace engineered organisms as an alternate route to an abundant utopia. At the same time, the rapid proliferation of personal computers and the increasing speed and complexity of integrated circuits in the late 1970s and the early 1980s stimulated interest in computer science and artificial intelligence among many young technophiles. Articles about machine intelligence and genetic engineering filled the pages of *Omni*, alongside fare about space colonization and parapsychology.

Carter's molecular electronics, then, was well timed to have crossover appeal to people who read and/or contributed to *Omni* and who were prepared to see computing and molecular biology as related domains. Probably the most influential member of this cadre was an O'Neill protégé named K. Eric Drexler, who, starting in the late 1970s, laid out a vision of a world remade by "molecular assemblers"—what today are often called "nanobots." Drexler cross-pollinated ideas from biotechnology and computer science to imagine programmable, molecular-scale automata able to construct any conceivable structure atom by atom. In 1986, Drexler would publish *Engines of Creation*, a bestselling proposal for how his assemblers would end resource scarcity while enabling interstellar travel, immortality, and transhuman cognitive and corporeal enhancement. As several scholars have shown, Drexler helped foster the emergence of the nanotechnology research field and of government nanotechnology initiatives in the United States and elsewhere. Along the way, he also elicited continual objections from scientists that his ideas were unrealistic and perhaps even harmful.[59]

At the time of the first MED conference in 1981, however, Drexler was only just beginning to move away from advocacy for space colonization and toward promotion of molecular nanotechnology. In that transition, Carter seems to have played a small but important role. Intellectually, some of Carter's ideas closely resembled Drexler's. For instance, Carter often wrote of fabricating a "'chemical' computer … through a series of computer-controlled chemical reactions in which chemical functional groups are added on a substrate one set at a time"—not far from Drexler's vision of feeding an instruction tape through a molecular assembler to program it to build a specific molecular structure.[60] Carter was also an active participant in a network (centered on the Santa Fe Institute) attempting to find a material basis to implement John von Neumann and Stephen Wolfram's theories of self-replicating "cellular automata."[61] Drexler traveled in related circles, and his molecular assemblers were self-replicating automata that similarly drew on the von Neumann formulation.[62]

Carter also may have provided Drexler with further education in the role of techno-prophet—though, having spent time in O'Neill's orbit, he may not have needed any assistance in that area. Finally, Carter introduced Drexler to the kinds of people who then helped manufacture Drexler's image as a public intellectual of molecular biomachines. For instance, Grant Fjermedal's book *The Tomorrow Makers: A Brave New World of Living-Brain Machines* describes how the author met one intoxicating "tomorrow maker" (Drexler) through another, more sober one (Carter):

> Now, if all of this seems to be too much in the realm of science fiction, it is interesting to note that I first heard of Drexler's work while at the U.S. Naval Research

> Laboratory, when Forrest Carter referred me to a paper of Drexler's that had been published by the National Academy of Sciences. … Indeed, it had been Carter who had prepared me for some of the ideas Drexler would present.[63]

To be sure, Drexler was merely one of several attendees who used the MED conferences as a platform for articulating extremely ambitious proposals for biocomputers and atomically precise manufacturing. For instance, Carter attracted interest from researchers at biotech and microelectronics start-ups who were eager to promote genetic engineering as a route to "biotechnical electron devices" or "biological assembly of molecular ultracircuits." Those were the titles of talks given at the first MED conference by Kevin Ulmer of Genex and James McAlear and John Wehrung of EMV Associates, all of whom would continue to attend Carter's workshops and would figure prominently in popular portrayals of molecular electronics in the 1980s. Kathleen McAuliffe mentioned them in *Omni*:

> "The ultimate scenario," says geneticist Kevin Ulmer of Genex Corporation, "is to develop a complete genetic code for the computer that would function as a virus, but instead of producing more virus, it would assemble a fully operational computer inside a cell." … The gemlike computer of McAlear's dreams, implanted in the brain, will sprout nerve projections from its tiny protein facets. … The biocomputer is not just another implant, he feels, but a symbiote, living from the cells it inhabits and giving them, in turn, the chance to evolve into a higher intelligence.[64]

As McAuliffe's final quotation from McAlear made clear, though, the "ultimate scenario" he had in mind went well beyond microcircuitry: "not only do I believe in an almighty God, but I'm probably the only one here that has any idea how to build it."

Nor were Drexler, McAlear, Ulmer, and Wehrung alone in staking molecular electronics' claims to something more radical than just moving past integrated circuitry. Through his MED conferences, Carter became one of the prominent hosts in a moveable feast of scientists and futurists promoting molecular electronics as the gateway to radical extension of human potential, the unlocking of consciousness, and the fabrication of new sentient beings. Two other hosts were Felix Hong and Michael Conrad of Wayne State University. Conrad wrote one of the first surveys of molecular computing (a trade report titled Biochips: A Feasibility Study); Hong went on to organize a 1988 Symposium on Molecular Electronics and edit its proceedings, *Molecular Electronics: Biosensors and Biocomputers*.[65] Both Conrad and Hong were regularly identified in the trade press as leading figures in molecular electronics. Conrad—trained as a biophysicist but housed in a computer science department—was also a high-profile figure in research on "quantum consciousness," occasionally pursuing that topic in collaboration with Brian Josephson (who became interested in consciousness as part of his advocacy of parapsychology, for which he had become almost as famous as for his work on superconductivity).[66]

Similarly, Stuart Hameroff, a University of Arizona professor of anesthesiology, attended Carter's conferences (and many other molecular electronics meetings) to promote his idea that quantum states in cellular microtubules are responsible for consciousness.[67] As Chris Toumey has shown, Hameroff's protégé (and Drexler's rival) Conrad Schneiker was one of the first to suggest that the scanning tunneling microscope might be a way to create and/or characterize Carter's molecular devices.[68] By the early 1990s, addressing single molecules with an STM would be a mainstream area of investigation; ten years earlier, however, Hameroff's enthusiastic talk about quantum consciousness—an area closely identified with parapsychology by both scientists and laypeople—probably put Carter's supervisors and many of the more conventional attendees at the MED conferences on edge.[69]

What proved Carter's undoing, then, was that his version of molecular electronics was increasingly seen by his supervisors and many in the scientific community as "too much in the realm of science fiction." As Mark Reed put it later, "I remember I met for the first time Eric Drexler there [at the third MED conference], and he was giving a poster and I remember having an argument with him at the time. Walking away shaking my head. … I just got kind of the impression that it's all theory here, no experiment, so I'm not going to take it seriously."[70]

Reed's description is ironic in view of his own association with visionary talk about molecular electronics in the 1990s. Still, Reed wasn't alone. Mark Ratner, for instance, gave a similar evaluation of the environment Carter had cultivated. The MED conferences, as he described them, "were enjoyable, and they were kind of crazy. … Have you read any of Forrest's papers? Then you know he had some really way out ideas about these soliton switches. He's nice and I like him, but I just couldn't see it—I didn't see how the ideas can be experimentally realized."[71]

Of course, Ratner had proposed a molecular rectifier that couldn't be experimentally realized at the time either. But the salient difference for Ratner, presumably, was that he approached the rectifier as a theoretical puzzle—a one-off attempt to imagine whether a single molecule could block current below a certain voltage and let it pass above that threshold. Carter, meanwhile, was trying to design an entire computer architecture made from molecules that couldn't yet be synthesized, much less individually addressed, and that were based on a phenomenon (soliton switching) that hadn't yet been shown to exist. Moreover, some of his recruits to molecular electronics were going even further by proposing molecular machines and computers that could think, live as symbiotes within human brains, enable human immortality, or substitute for "almighty God."

The dangers of such far-reaching proposals began to catch up with Carter in May of 1983, when the para-scientific press reported on his second MED conference. First, *Science* noted that

> A recent workshop on the subject [of molecular electronics] at the Naval Research Laboratory in Washington, D.C., showed that there is a considerable gap between the cinemascope pictures of the thinkers [e.g., a "human-computer hybrid"] and what they can do right now. … Forrest Carter, the Navy laboratory chemist who has been energetically promoting the concept of molecular electronics, smiles but does not disavow such visionary applications when asked about them.[72]

The same month, *Chemical & Engineering News*—a widely read trade journal for American chemists and chemical engineers—published an article in a similar vein. Though the tone was generally positive, some of its language had negative implications:

> A small but zealous group of chemists is convinced that the electronics and computer industry could benefit enormously by being brought down to the molecular level. … Such talk is not mere science fiction. Some of it has already moved into the realm of theory. … [Carter] caution[s]: "There's an enormous job ahead. Yet, it's so exciting and vital that I'm sticking my neck way out."[73]

The words "gap," "visionary," "zealous," and "science fiction" aren't pejorative on their own, of course, but taken together they indicate an ambivalence about Carter's project.

Six months later, *C&EN*'s ambivalence slid toward disapproval with another article, "Molecular Computers Are Far from Realization," describing yet another molecular electronics conference:

> [H]ype seems inevitable for so-called molecular computers. There are claims for the miraculous things they will be able to do compared to present computers. … Such computers currently are barely a step removed from science fiction. … But such reservations tend to get muted when a group of scientists get excited about new ideas. … The National Science Foundation funded the conference to find out whether research into chemically based computers is a promising field to support. The straightforward answer to that question, even the enthusiastic conference participants admitted, is: Not really. But that simple answer was stated in muted terms, drowned in a succession of "gee whiz" tales.[74]

That article highlighted Carter as the most egregious of the overly "enthusiastic conference participants." A month later, *C&EN* published a letter from Edwin Chandross, a Bell Labs photoresist chemist, excoriating Carter and molecular electronics[75]; it drew indignant replies from Carter and one of his protégés.

The exchange in *C&EN* showed that, for much of the scientific community, molecular electronics' American proponents were not believable. As the British chemist Richard Jones puts it, the field had gained "in the USA, a persistent louche reputation."[76] Or, as Jones has quoted physicist John Hopfield elsewhere, "the field suffers from an excess of imagination and a deficiency of accomplishment."[77] The broadcasting of that reputation in late 1983 was especially poorly timed for Carter because it came just as changes in the NRL's leadership led to stricter oversight of his work. In February of 1984—three months after the second *C&EN* article and two months after Chandross' public denunciation—a new superintendent of the chemistry division, William Tolles, arrived at the NRL. Tolles reviewed Carter's publications and the scientific community's reaction to them with some alarm and put Carter under closer supervision. In the final three and a half years of Carter's life (he died from a brain tumor in December of 1987), his network building slowed as the NRL withheld the institutional support it had once provided. Though Carter did organize one last MED conference (held in October of 1986), he did so without sponsorship from his employer.[78]

Carter's story gives a good sense of the limits and capabilities of the network-failure paradigm of state intervention in microelectronics research, particularly in comparison with the market-failure approach taken by the National Security Agency. Clearly, Carter understood that he didn't have access to state resources that would drag a molecular computer into existence, and that therefore his energy was better spent connecting together actors with different kinds of relevant expertise who, as a networked collective, might bring that aim closer to reality. Organizing and attending conferences, editing volumes, getting one's ideas into the popular and parascientific media—these are the tools of ameliorating network failure that Carter wielded, as did similar actors with similar aims whom we will meet in later chapters.

Two things undercut Carter's approach, however. First, his supervisors didn't view his network-building activities as a proper use of his time, especially after the *Chemical & Engineering News* debacle of 1983. As we will see in chapter 6, if Carter had been at a different agency, especially DARPA, such activities might have been seen as part of his job, rather than as distractions. Second, the network-failure approach works only if state intervention results in greater trust between nodes in the network. Carter's open-door policy certainly expanded the network interested in molecular electronics, and he was able to locally increase trust among a few participants. However, by trying to keep the molecular electronics network together through his own personality, and by lending legitimacy to other visionaries, Carter paved the way for a general *loss* of trust between American molecular electronics and much of the scientific community.

What Might Have Been, and What Was Elsewhere

As we'll see in chapter 6, the louche reputation of American molecular electronics that emerged under Carter's leadership has endured into the twenty-first century. Even in Carter's lifetime, though, the field was not quite as devoid of experimental accomplishment as some of its skeptics claimed. For instance, Bob Metzger, a chemist at the University of Mississippi (and later at the University of Alabama), had set himself the objective of synthesizing an Aviram-Ratner-type molecular rectifier, and reported various milestones at the MED conferences—though he didn't succeed until 1997, almost a quarter of a century after the original Aviram-Ratner proposal.[79] For Metzger, as for many in the molecular electronics community in the 1980s, Carter's conferences had been a valuable forum for reporting results, even though those results were relatively meager. As Metzger put it in 1999, "in the early 1980s, sparked by three scientific conferences organized by the late Forrest L. Carter, the idea of 'molecular electronics,' that is, electronic devices consisting solely of molecules, gained large-scale interest."[80]

Another circle of experimentalists who adopted "molecular electronics" as an aim in the 1980s were molecular biologists and biophysicists interested in rhodopsin, a photosensitive protein found both in bacteria and in the retinas of vertebrates. As Mathias Grote has shown, rhodopsin's unusual photochemical properties had attracted a low level of scientific interest for a long time, only to suddenly spike in the 1970s.[81] Grote argues that most of those working on rhodopsin in the 1970s were outside the contemporary mainstream of molecular biology, with its focus on DNA and genetic regulation of cellular processes and protein expression. Instead, rhodopsinists were interested in biological "stuff that does something" at the molecular scale.

By the 1980s, rhodopsinists had come to conceive of their object of investigation as a complex system for receiving light and responding to it—or, to put it slightly differently, for receiving *signals* and responding to them. Hence, rhodopsinists such as Robert Birge (of Syracuse University) eagerly attended Carter's MED conferences as a means for moving from the margins of molecular biology to the center of an expanding network of biocomputing researchers.[82] After all, one oft-proposed solution to the limits on continued scaling of integrated circuits has been to replace metallic connections between transistors with optical ones, thereby allowing faster movement of signals with less power consumption.[83] In joining the molecular electronics community, Birge and his colleagues put rhodopsin forward as a means of enabling such optical connections, or even as a step toward optical information processing *sans* silicon entirely.

The problem for American molecular electronics, then, was not that no experiments were being done, but rather that the field's most vocal practitioners had their eyes on very ambitious end goals—molecular computers, biocomputers, artificial symbiotes, even immortality—with little evidence that current experimental capabilities would lead to those ends. The grandiose character of molecular electronics' long-term future counted against the field's short-term prospects. Things could, however, have been otherwise: Far-off, grandiose promises could have warranted the funding of near-term, incremental research, and near-term research could have been taken as evidence that ambitious objectives had a chance of being achieved. The possibility of such an alternative path is demonstrated by the fact that outside the United States communities dedicated to molecular electronics research formed and made progress without gaining a "louche" reputation.

The term "molecular electronics" was taken up all over the world around 1980. Researchers in Japan were among the first to use it, primarily as an umbrella label for a variety of projects in molecular biology and biocomputing roughly along the same lines as the projects of Robert Birge and other rhodopsinists. There was a large contingent from Japan at Carter's MED conferences, and some of the earliest conferences on molecular electronics outside the United States were held in Japan.[84]

As also happened with more conventional microelectronics, money for research in molecular electronics in Japan flowed from the Ministry of International Trade and Industry.[85] One major difference from MITI's sponsorship of the VLSI program in the mid 1970s, however, was that in the mid 1980s the Japanese government was under pressure (primarily from the United States) to share with other countries the burden of long-range, basic research, and to distribute government research funding in such a way that it wouldn't constitute an unfair advantage for Japanese firms. As a result, Mort La Brecque wrote, "one unusual aspect of the [molecular electronics] project is that as many as 30 percent of the participating scientists are not required to be citizens of Japan."[86] Both Kevin Ulmer and Tony Garito (Alan Heeger's former postdoc from the early TTF-TCNQ days) headed laboratories within the MITI molecular electronics program.

There was also an active network of molecular electronics researchers in Warsaw Pact countries, especially Poland, Hungary, and the Soviet Union. At least from the perspective of scientists in the West, Soviet work on molecular electronics tended to be insular and disconnected from the global community; as Michael Conrad observed, "things are very Moscowcentric in the Soviet Union … so there are lots of affiliated institutes throughout the country engaged in similar research activities under the general authority of the central program."[87] In Poland and Hungary, by contrast, molecular electronics was, in part, a means for scientists to forge ties with the rest of the world, especially Western Europe. Several Polish scientists, for instance, collaborated with an English chemist, Bob Munn, on molecular electronics projects, and as a result there were exchanges of personnel in both directions.[88] Hungarian scientists, meanwhile, organized an international conference on molecular electronics in 1988, which spawned successor meetings in Moscow in 1989 and New York in 1991. In 1991, with help from Michael Conrad, the Hungarian group founded the International Society for Molecular Electronics and BioComputing, which sponsored further conferences for a few years in the 1990s.[89]

Molecular electronics in the United Kingdom, though, provided the clearest contrast with—and alternative to—the path taken in the United States. The central figure in early British molecular electronics was the Welsh physicist Gareth Roberts, who shared Forrest Carter's talent for network building but who seems to have been more proactive than Carter in discouraging over-promising and experimental under-achieving. Whereas Carter was almost always described as kindly or smiling, Roberts' obituary noted that he "was a genial, humane man, but his twinkling eyes could rapidly turn grey if he was crossed. … Blessed with superhuman energy, Roberts was perhaps less tolerant than he might have been of the weaknesses of others."[90]

Like Ari Aviram and Philip Seiden, Gareth Roberts came to molecular electronics in part via copier technology. Trained as a condensed matter physicist, he was hired at Xerox's lab near Rochester, New York in 1966, largely to work on photoconductivity in selenium.[91] Presumably through that work, Roberts, like the members of IBM's molecular electronics group, became interested in organic conductors, and he developed a research program in that area when he took a post at New University of Ulster in 1969.

By 1976, when he took a position at Durham University, he had turned his attention to Langmuir-Blodgett (LB) films—organic films that can be made very thin and with very precise thickness and can be overlaid on an inorganic substrate. Although LB films were invented in the 1930s for non-reflective coatings (among other things), Roberts was "a pivotal figure in inspiring many groups worldwide to adopt Langmuir-Blodgett techniques as a method for producing thin film organic structures," especially for novel electronic devices.[92]

One of the hallmarks of Gareth Roberts' career was the ease with which he oscillated among industrial, academic, and government institutions, tying their interests together. Before Ulster and Durham he had worked at Xerox; during that academic stint he consulted extensively for ICI (Imperial Chemical Industries); from Durham "he left for joint appointments as Director of Research, Thorn EMI … and Visiting Professor of Electronic Engineering at Brasenose College, in Oxford."[93] Thus, his academic work was thoroughly informed by the needs of industry, particularly the needs of firms involved in (or wanting to be involved in) electronics. For instance, "he appreciated early the need for students to understand silicon chips, and established at Durham one of the first UK undergraduate foundries," in order to acquaint students with techniques used in microelectronics firms.[94]

By the late 1970s, therefore, Roberts was well placed to begin assembling a network of industrial and academic researchers with a common interest in using organic molecules for microelectronics. As in the parallel efforts coalescing in other countries, there was no necessity that Roberts and his comrades should label what they were doing as "molecular electronics." Perhaps Roberts had picked up that term from Carter's reports for the NRL's Electroactive Polymers program or from invitations to the first MED conference, or even from the earlier Westinghouse era; perhaps he re-coined it himself. In any case, by October of 1980—several months before Carter's first MED conference—Roberts took an existing group of (mostly Welsh) academic and industrial researchers who met annually for an informal get-together and transformed it into a "Molecular Electronics Discussion Group" (MEDG).[95]

The MEDG soon attached itself in an advisory capacity to one of the United Kingdom's main bodies for science policy and science funding, the Science and Engineering Research Council. In 1984 that advisory role was formalized when the MEDG became the Molecular Electronics Advisory Group (MEAG). Notably, the Advisory Group was almost equally split between academics and representatives of large firms (among them ICI, Plessey, British Telecom, Pilkington, and Unilever). The MEAG, in turn, persuaded the government of the United Kingdom to invest £20 million in molecular electronics through the new LINK program, one of the Thatcher government's favored tools for promoting greater university-industry collaboration.[96]

Roberts and British molecular electronics researchers collectively were well equipped to take advantage of calls for university-industry cooperation. As the introduction to the MEAG's 1985 report to the Science and Engineering Research Council put it,

> Molecular electronics has evolved over the last fifteen years or so mainly as an answer to the relentless pressure from the electronics industries for continuous reductions in the sizes of components to give higher operating speeds and packing densities and their continual demand for new components, e.g. displays, sensors and actuators, which enable new products and systems to be produced. This industrial pull adds value and utility to the exciting achievements and discoveries which can be made by addressing the substantial challenges of the difficult and varied science involved.[97]

That is, Roberts and his allies framed molecular electronics as lying exactly at the sweet spot where long-range, basic research (of the type academic researchers are equipped to do) can inform and be informed by short-term and medium-term industrial problems. That conjunction between the capabilities and needs of university and industry was less evident in American molecular electronics in the 1980s.

As the quotation above indicates, Roberts and the MEAG invoked Moore's Law and the limits to integrated circuit scaling as the paramount industrial problem that molecular electronics researchers had their eyes on. But he and his committee were also careful to point to other kinds of molecular electronic devices—"displays, sensors and actuators"—that were not as dominated by Moore's Law or by silicon, and therefore where British firms were better-positioned to compete than in the mass production of ICs. Thus, British molecular electronics was never as suffused with talk of "molecular computers" or "ultimate limits" to miniaturization as its American cousin. Nor did British molecular electronics researchers talk as much as their counterparts in the United States and Japan about biocomputing, molecular machines, artificial symbiotes, and other organism-like devices. After all, British biotechnology firms were not yet associated with the kind of economy-saving hopes that their American rivals were associated with.

To be sure, there was some talk in the United Kingdom about molecular computers and biomolecular electronics. However, Roberts cordoned off such talk in the MEAG's "Molecular Electronics 'Futures' Group" led by John Owen "JO" Williams, a Welsh solid-state chemist at the University of Manchester's Institute of Science and Technology. Several of the members of that group came from centers for research in molecular biology and in biotechnology, whereas the rest of the MEAG was composed largely of chemists, physicists, and electrical engineers. And, unlike the other subcommittees of the MEAG, the futures group took up the task of exploring Carter's ideas, though with the caveats that "many of Forrest Carter's original suggestions in the field of molecular electronics were not well received" and that "the Group recognizes that the subject presently appears futuristic and that a truly molecular computer may never be achieved."[98]

Despite all its notes of caution, the Futures Group was still quite marginal in shaping British molecular electronics. In 1986, Jim Feast, a Durham chemist, wrote the following in a report to his local colleagues:

> I attended a "Molecular Electronics Town Meeting" organized by the SERC at the Royal Society. … Roberts' talk was well received and there was general approval for his considerable efforts to promote the subject. Professor JO Williams [of the University of Manchester Institute of Science and Technology] then presented something called the "Futures" Group Report, it proved difficult to understand what, if anything, this presentation was all about. My difficulty was shared by many of the audience and this talk was not well received.[99]

The Futures Group's importance, though, may have had less to do with the feasibility of its conclusions than with its ability to offer an attractive vision to government funders that would then benefit researchers in areas in which results were more likely to be forthcoming. Jim Feast captured a bit of that sense later in his report:

> In the afternoon there was a general discussion of … the Plan for Action. The amount of critical comment was remarkably low for an assembly with a high proportion of academics; perhaps too low. The discussion was mostly warm with approval (accompanied, I felt, by the sounds of cash registers ringing in many ears).[100]

In other words, the British molecular electronics community (under Roberts' direction) navigated between the less-than-critical promises of Forrest Carter and the more-than-critical disparagements of Edwin Chandross. The "louche" segments of the British molecular electronics community could be used to entice funders, but with enough caveats that they were never taken as representative of the community as a whole. Instead, the

MEAG made sure that the tent of "molecular electronics" was large enough that it included the prize of molecular computers but also a wide menu of near-term, eminently achievable research goals that both funders and fundees could agree should be given resources. Those goals included "liquid crystals, Langmuir-Blodgett films, organic conductors, microlithographic resists, photochromic and electrochromic materials, pyroelectric and piezoelectric materials, chemical and biological sensors, non-linear optics, [and] metal organic chemical vapour deposition."[101]

With "cash registers ringing" in their ears, British molecular electronics researchers built an impressive array of institutions in the late 1980s. Roberts' former colleagues at Durham, for instance, founded a Centre for Molecular Electronics in 1987, which allowed them to win a series of grants, to build a clean room, and to accrue experimental equipment for research on LB films, conducting polymers, liquid crystals, and other areas recommended by the MEAG. The Durham center also raised funds by holding periodic "short courses" on various topics in molecular electronics, primarily for researchers from industry.[102] Other universities—often in concert with one another or with industry—also formed molecular electronics centers. As a result, the British molecular electronics network grew, and community-wide institutions began to appear. For instance, the director of the Durham center, Mike Petty, "was made editor of a new SERC/DTI [Department of Trade and Industry] newsletter concerning molecular electronics" beginning in the summer of 1988.[103] Even earlier, in 1985, Robert Munn at the University of Manchester Institute of Science and Technology had founded a *Journal of Molecular Electronics* to publish peer-reviewed articles on the topic.

Such institutions differed significantly from American molecular electronics, which had no real institutions apart from Forrest Carter's MED conferences (which didn't outlive their founder). Also, in the United States, association with molecular electronics' louche reputation ended Carter's career, whereas in Britain molecular electronics boosted Gareth Roberts on a career trajectory that included the vice-chancellorship of Sheffield University, the presidency of Wolfson College at Oxford, and chairmanships of numerous national science-policy committees. To this day, British scientists use the term "Roberts money" to refer to a stream of government funding for career development and training in new areas that Roberts helped establish.

Yet there was one important similarity in the evolution of the field in both countries, and to some extent globally. More or less everywhere, molecular electronics paused between the end of the Cold War and the late

1990s. That hiatus—similar to what Terry Shinn and Anne Marcovich have referred to as a moment of "respiration"—can be seen clearly in figure 3.1. Shinn and Marcovich describe "respiration" as "a pause during which scientists evaluate what new combinatorials are required to extend their present research, or when they study the cognitive horizons possibly available in adopting a different set of combinatorials."[104] For Shinn and Marcovich, respiration is largely an individual caesura. But if molecular electronics is any indication, collectives of scientists also can "respire."

In the United Kingdom, we can discern a moment of respiration in the gradual decline of the SERC-DTI newsletter (with an ultimate demise in the autumn of 1993). The year before, the *Journal of Molecular Electronics* also ended its run. By the early 1990s, the Science and Engineering Research Council's funding stream dedicated to molecular electronics had dried up, leaving researchers to recast what they were doing in order to take advantage of new opportunities. At the same time, the large British chemical and electronics firms that had participated in the Molecular Electronics Advisory Group were beginning to shed their long-range research capacity in order to compete more nimbly. Since molecular electronics had always been a side bet for these companies, it was one of the first commitments to be cut.

In the United States, respiration occurred partly because of Forrest Carter's death and partly because the field was widely seen as a branch of futurism rather than as empirical research. Though many of those responsible for the field's reputation—Ulmer, McAlear, Hameroff, Drexler, and others—remained active into the 1990s, without Carter this cadre employed "molecular electronics" less and less as their rallying cry. Instead, "nanotechnology" took its place, especially after the publication of Drexler's books *Engines of Creation: The Coming Era of Nanotechnology* (1986) and *Nanosystems: Molecular Machinery, Manufacturing, and Computation* (1992).

While the more speculative wing of molecular electronics veered off in the direction of nanotechnology, a new generation of laboratory experimentalists coalesced around the third iteration of molecular electronics. As we'll see, some figures from the second generation were instrumental in that renaissance—particularly Ari Aviram and, a little later, Mark Ratner. It took a while, though, for the third iteration to get going; hence the mid-1990s moment of respiration. That hiatus wasn't sufficient for American molecular electronics to shake its reputation, or for the community to change the habits that had led to that reputation, but it did allow for the growth of institutions and funding streams of the type seen in Britain and Japan in the 1980s. By the time molecular electronics got going again in the United States, though, the world and American science were very different. The untried institutional innovations for research that emerged in the late days of the Cold War had become the stable new reality after the Cold War, forcing adaptation in every research community, not least molecular electronics.

4 New Institutions for Submicron Research

As we saw in chapter 1, the early 1970s were a period of multiple, compounding crises and exploratory experimentation in American research universities. A variety of local stakeholders, including student activists, surrounding communities, nearby firms, trustees, and restless faculty members, worked to deconstruct, reassemble, and invent academic research institutions. The same stakeholders also struggled to cope with the institutional deconstructions and innovations put forward by others. As was noted in chapter 1, successful academic researchers had to find ways of defusing pressure from activists, while strategically enrolling a variety of partners (users, civil society groups, interdisciplinary collaborators, federal agencies, firms, philanthropic organizations) in order to overcome deficits of money and legitimacy.

This chapter reverses perspective to look at how institutional innovations emanating from federal agencies (and from IBM and other firms with close ties to the state) oriented academic researchers toward projects that furthered the state's aims. Of course, the same budgetary and legitimacy crises that afflicted universities also constrained and enabled action by federal agencies. Within the federal research apparatus, some bureaucratic units and some individual civil servants navigated the fluid political and fiscal environment of the 1970s by experimenting with new ways of funding and guiding American science. These experiments were, in a sense, reactive—they would not have occurred if not for the tectonic changes going on around them. But some of these institutional innovations went beyond mere reaction to establish a new set of tools that would allow research organizations to make themselves party to future moments of technopolitical controversy without being existentially threatened by those controversies in the way Stanford University and its peers had been in the late 1960s.

Institutional innovations put forward by federal agencies prospered when they were mutually reinforcing with innovations within universities.

Of course, complete alignment between academic and state initiatives was rare, especially on the first attempt. But good-enough alignment could lead to an innovation's being copied repeatedly across the landscape of American science. This chapter will examine one such good-enough institution: the academic microfabrication facility. These facilities, which house tools similar to those used in the semiconductor industry and rent them out to users from universities, government, and industry, were almost unheard of before the late 1970s, but have become critical "infrastructural" organizations for American research over the past forty years.

Professionalization, Sophistication, Global Competition

The construction of an organizational field of academic microfabrication facilities began in 1974, when the sixteen program officers in the Engineering Division of the National Science Foundation were instructed to propose a marquee project that would cement engineering's growing place within the United States' premier non-medical civilian science funding agency. Discussions regarding the Engineering Division's marquee project were connected to several significant changes then underway at the National Science Foundation. Before 1970 it had been a small funder, mostly of individual basic researchers in the physical and life sciences. But in the 1970s it began to put more emphasis on engineering and social sciences and on interdisciplinary applied research, and to channel more of its funding through university-run centers rather than funding individuals directly.[1]

The NSF's turn toward center-based funding in the 1970s was due in part to its acquisition of a dozen Materials Research Laboratories (MRLs—the renamed Interdisciplinary Laboratories) from ARPA after the Mansfield Amendment, which barred the Department of Defense from funding basic research, was passed. By adopting the MRLs, the NSF also imported the lesson that academic centers could be used to broker interdisciplinary collaboration, as was noted in chapter 3's examination of the University of Pennsylvania's MRL and the conducting polymers boom of the mid 1970s. Similarly, in astronomy, the NSF inherited the Arecibo Observatory from ARPA in 1971 and the Sacramento Peak Observatory from the Air Force in 1976. Yet another facility transferred to the NSF because of the Mansfield Amendment and cuts in the military budget was MIT's Francis Bitter National Magnet Laboratory.[2] The FBNML was founded in 1961, with an Air Force Office of Scientific Research grant, in order to provide powerful electromagnets to visiting researchers from around the country. By adopting the Bitter Lab, the NSF imported an organizational model for a "national" user facility which was explicitly drawn upon in the Engineering Division's marquee project discussions in 1974.[3]

As it absorbed these formerly military-sponsored centers, the NSF also started to launch its own equipment-providing sites along similar lines. In 1973, for instance, the NSF helped the Stanford Synchrotron Radiation Project become a "national user facility" renting synchrotron beam time to materials scientists, microelectronics researchers, and molecular biologists.[4] The same year, the NSF founded a Biological Research Resources Program to support "systematic biology collections in museums and botanical gardens."[5] And in chemistry, the NSF received authorization in 1978 to fund a series of Regional Instrumentation Facilities, fourteen of which were renting out experimental equipment by 1982.[6]

The NSF of 1974, therefore, was a favorable place for the Engineering Division's program officers to propose some kind of interdisciplinary academic research center and/or national shared-equipment or "research resource" facility as a marquee project. Charles Polk, head of the Engineering Division in 1976–77, put it this way:

> We have talked about a national center or several regional laboratories where that major, expensive equipment would be available. … The large initial investment and the continuing support which are required could be justified only in terms of benefits to many research workers and to many different institutions. As a consequence, a national or regional laboratory, supported by NSF, would have to make very good provisions for guest workers and would have to engage permanent personnel which would help visitors with physical implementation of their ideas.[7]

The only questions concerned which program officer's proposal best embodied these principles and which field of engineering should be served by such a facility.

Three interrelated trends pushed Polk and his colleagues to select a proposal in the field of microfabrication, a research area devoted to making tiny engineered devices—such as transistors—and therefore a field closely linked with Moore's Law and the microelectronics industry. Those trends were the professionalization and increasing visibility of microfabrication research, the increasing technical sophistication and expense of microfabrication equipment, and growing international competition in semiconductor manufacturing (especially from Japan). Professionalization was a product of a suite of new institutions that microfabrication specialists had established to make their work more visible to one another and to outside actors such as NSF officers. That visibility gave officers in the NSF's Engineering Division reason to think that microfabrication was an up-and-coming field, that both science and the nation would benefit from the sharing of equipment, and that the NSF would get maximum credit for providing that equipment.

We can track the professionalization and the increasing visibility of microfabrication by looking at some of the conference series that microfabrication specialists established to facilitate co-presence and sharing of information. In the mid 1960s, the Institute of Electrical and Electronics Engineers and the Electrochemical Society began sponsoring competing conferences on the use of electron beams for carving out microelectronic devices. In 1967, two specialists in electron-beam lithography, Tom Everhart from the University of California at Berkeley and Fabian Pease from Bell Labs, made a "gentleman's agreement" to alternate the two conference series.[8] The purview of these meetings then expanded to include other microfabrication techniques until the new, alternating conference series became known informally as the "Three Beams meeting"—a name derived from the electron-beam, ion-beam, and photon-beam (both optical and x-ray) lithographies that most attendees employed. (For an explanation of how beam lithographies work, see figure 4.1.) Then in 1973, the Three Beams meeting got a formal steering committee thanks to that year's program chairman, Edward D. Wolf from Hughes Research Labs. Before 1973, presentations at the Three Beams meeting "were not refereed and appear to have been 'self-published' with a very small circulation"; after that year, proceedings were peer reviewed and were published in the American Vacuum Society's *Journal of Vacuum Science and Technology*.[9]

Everhart, Pease, and Wolf will all be major figures in this chapter and the next, in part because their institutional innovation in the microfabrication research community positioned them to take important roles in the first wave of academic microfabrication facilities. But their institutional innovations were entangled with a technological revolution in microfabrication in which they were also leading participants. The merger of the IEEE's and the Electrochemical Society's electron-beam lithography meetings, the expansion of those conferences to include other lithographies, and the peer-reviewed publication of the Three Beams proceedings were all necessary because of that technical revolution, which constituted the second trend in microfabrication research visible to the NSF. Critically, the rapid innovation in microfabrication tools that occurred in the early 1970s also led to a sharp increase in the cost of those tools. Indeed, it was primarily because of the increasing expense of tools for microfabrication that the NSF perceived a need for a facility where users could rent, rather than buy, those tools.

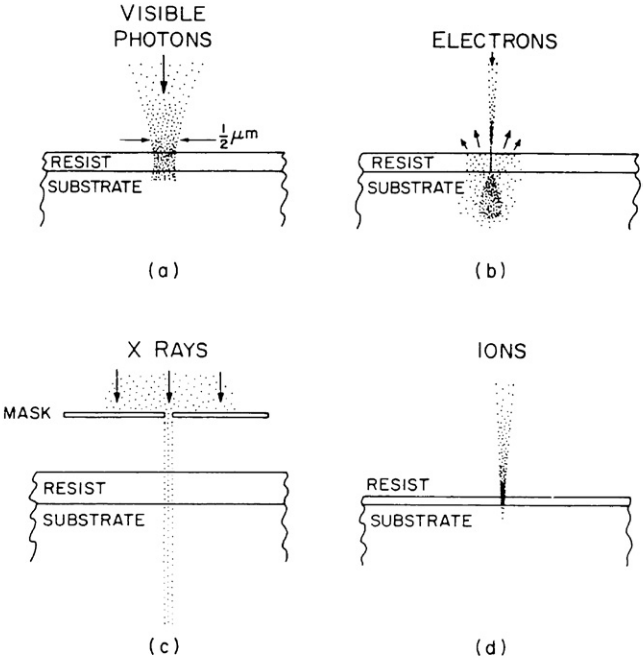


Figure 4.1

The "three beams": photon beams, both optical (a) and x-ray (c); electron beams (b); and ion beams (d). The beams shine through a template (a "mask") onto a substrate that has been coated with a varnish-like "resist." Where the beam hits the resist, it changes its chemistry, giving it either greater or lesser resistance to acid. Thus, when the resist is washed in acid, the pattern in the mask will be etched into the substrate. This is how complicated circuit patterns are transferred into silicon wafers in semiconductor manufacturing. Reprinted from E. D. Wolf and J. M. Ballantyne, "Research and Resource at the National Submicron Facility," in *VLSI Electronics: Microstructure Science*, volume 1, ed. Norman G. Einspruch (Academic Press, 1981), copyright 1981. Used with permission from Elsevier.

Today it seems almost unimaginable that microfabrication would *not* be an expensive high-technology enterprise. Modern semiconductor manufacturing wouldn't be possible without multi-million-dollar tools located in multi-billion-dollar "fabs" that maintain the cleanest, most precisely controlled conditions on Earth. But well into the 1970s advanced microelectronic devices were still often made by hand, especially for academic research. In a 2010 interview, Alan Kleinsasser, who had been a graduate student at Cornell and who later had worked on the IBM Josephson project, described the state of the art in the mid 1970s: Some people in his thesis adviser's research group "were trying to define submicron Josephson junctions using techniques like scratching films with a razor blade. [Or] you could take a glass rod, and hold it over a Bunsen burner, and if you just sort of pull it apart, you'll get this thinning piece of glass. Well, that leaves you with micron pieces of glass at the ends. I remember one postdoc collecting these pieces on the table and identifying under a microscope which ones to use as an evaporation mask, to define small dimensions that way. [People were] doing lithography [by] shining light through an optical microscope to focus it. And of course there was rubylith for mask-making where you're literally scribing patterns with razor blades."[10] Even some commercial circuits were still made in relatively low-tech ways in the early 1970s. For instance, Robert Noyce sometimes brought his children to Intel so that they could hand-cut commercial circuit patterns out of rubylith films for fun.[11]

By the end of the 1970s, though, crude lithographic techniques were practically extinct in industry. Various improvements had appeared, or were in development, that made optical lithography better for industry but too expensive for most academic researchers. These included better stepper motors for positioning masks correctly, better clean rooms and more rigorous control of workers in those rooms, new photoresists, and new means of eating away at the resist and substrate (among them plasma etching and reactive ion etching).

At the same time, *alternatives* to photolithography pushed the limits of industrial microfabrication even further. For instance, since the resolution of optical lithography is limited by the wavelength of light shone on the photoresist, some researchers advocated moving to x-ray and "extreme ultraviolet" (EUV) radiation, which has shorter wavelengths and, on the face of it, a smaller ultimate feature size. In 1995 the management scholar Rebecca Henderson estimated that "IBM alone has invested over a billion dollars in x-ray lithography."[12] Other researchers, including Pease, Everhart, and Wolf, believed that a similar improvement in resolution could be had by switching from photon to electron beams. Bell Laboratories developed an e-beam writer called the Electron Beam Exposure System (or EBES), and AT&T's manufacturing subsidiary, Western Electric, had installed it in its fabs by 1976. Perkin Elmer, Varian, and other firms later licensed the EBES technology and sold their own versions to semiconductor manufacturers.

E-beam lithography found a lasting niche in semiconductor manufacturing as a tool for making masks for optical lithography. That is, it is used to make templates for commercial integrated circuits, but it generally isn't used in writing circuit patterns directly. However, well into the 1980s many researchers were confident that electron beams *would* be used to directly write commercial circuit patterns into silicon, and/or that x-ray photolithography machines were on the verge of displacing optical lithography in commercial manufacturing. For instance, in 1983 Ian Ross, president of Bell Labs, offered this typical assessment of the near future of semiconductor manufacturing:

> The first challenge, of course, is to continue to reduce the size of elemental components. This depends largely on our ability to improve lithography. … It is estimated that, using visible light and all of the tricks we can conceive of, we might ideally get down to line widths of a half micron. Under practical conditions, however, this is more likely to be 1 micron. Now, beyond that one can go to ultraviolet light. This is merely going to shorter wavelengths, which under laboratory conditions possibly would take us down to a quarter micron, or a half micron in practice. The next step is to go to x-ray lithography using even shorter wavelengths. Theoretically, this might even take us down to a tenth micron. But most likely the ultimate technique, one that has already been used in mask making, is to use electron-beam lithography. And there one can predict, and indeed can demonstrate that lines within the range one-tenth to one-hundredth micron are possible.[13]

Here we see the interpretive flexibility of Moore's Law's social fact-ness on display. For Ross and his audience, it was simply a fact that "the size of elemental components" would continue to get smaller. But Ross also took it as fact that optical lithography would not extend miniaturization much further, and that therefore alternative lithographies *must* take its place.

For Intel and other Silicon Valley firms, however, the factuality of optical lithography's demise was in dispute. In fact, miniaturization has proceeded even further than Ross predicted, mostly on the basis of improvements in optical lithography—and not, as Ross thought, via adoption of alternatives such as electron beams and x-ray beams. The context for Ross' speech, therefore, was what one participant has called "the 'lithography wars'—a titanic struggle for supremacy among a dozen or so competing lithographic technologies, the winner of which was assumed would inherit the mantle of optical lithography in the manufacturing of integrated circuits."[14] The new institutions of microfabrication, such as the Three Beams meeting, arose in part as arenas in which those lithography wars could be fought.

In the mid 1970s these two entangled trends—the institutionalization and the increasing sophistication of microfabrication—were given further urgency at the National Science Foundation by a third trend: dramatic gains in competitiveness of Japanese microelectronics firms relative to their American peers. In some ways, this turn of events should not have been unexpected. Japanese firms had been making transistors since the beginning of the 1950s, and as early as 1959 Japan's semiconductor industry had occasionally produced more transistors in a year than its US counterpart.[15] Still, the advent of integrated circuits had seemed to guarantee American dominance of the semiconductor industry—until the early 1970s. As American firms' share of the market for dynamic random-access memory (DRAM) chips slid from 95 percent in 1971 to 29 percent in 1979 (shifting mostly to Japan), panic set in at the highest levels of American government and industry.[16]

That panic was acutely sharpened by Japan's announcement in 1975 of a government-led crash program in very-large-scale integrated (VLSI) circuits. The VLSI program was coordinated by the Ministry of International Trade and Industry (MITI), with different projects parceled out to teams led by different manufacturing firms. Even today, it isn't clear how successful the MITI VLSI program was at fostering Japanese firms' competitiveness.[17] At the time, however, Japanese firms' increasing market share led observers in the United States to believe that "unless U.S. technology advances … the U.S. market itself could be under Japanese domination."[18] As Richard Langlois and Edward Steinmuller put it, looking back on this era from 1999, "shrill cries arose … that the American semiconductor industry would soon share the fate of the lamented American consumer electronics business. Few dissented from the implications: the only hope for salvation would be [for federal agencies such as the NSF] to adopt Japanese-style public policies."[19]

Three Workshops and a Conference

By 1975, then, the pieces were in place to make an academic microfabrication user facility the most promising candidate for the marquee project of the National Science Foundation's Engineering Division. On the one hand, the MITI VLSI program put pressure on the NSF to aid the American semiconductor industry. On the other hand, many units within the NSF were worried that research tools—including in the rapidly professionalizing field of microfabrication—were becoming prohibitively expensive for universities.[20] Indeed, the NSF program officer who put forward the idea for a "National Research and Resource Facility for Submicron Structures" (NRRFSS), Jay Harris, had firsthand experience of being priced out of state-of-the-art microelectronics research.

As Harris recalled later, when he was a faculty member at the University of Washington, before he worked at the NSF, he "used to visit various industrial laboratories to try to get some help in making small optical structures." "I got my best reception at the Hughes research labs in Malibu," he continued, “from a guy named Ed Wolf, who was working with electron beams, but Ed didn’t really have time to devote to supporting academics trying to work over their heads.”[21] Wolf concurred that by 1975 “a very noticeable gap [had] opened between university research on the one hand and the accomplishments of industrial laboratories on the other—a gap due mainly to the expensive equipment and the interdisciplinary nature of microstructure science and engineering that universities found difficult, if not impossible, to support.”[22] Harris’ bosses at the NSF, who were from similar academic engineering science backgrounds, strongly supported his proposal. Even Guyford Stever, the NSF’s director (and the presidential science adviser), enthused that the NSF was “establishing a National Microfabrication Institute [i.e., the NRRFSS], since microfabrication holds great promise and is a field in which the United States could excel and enjoy an important competitive advantage industrially.”[23]

However, some members of the National Science Board, the NSF’s governing body, were less supportive and demanded more evidence from Harris that such a facility was really needed. One reason the NSB was skeptical was that it wasn’t yet fully comfortable with the center-based funding model. Some members worried that Harris’ plan for a large block grant to the university hosting the facility would attenuate the NSF’s oversight of how its money would be spent and undermine the NSF’s traditional commitment to meritocratic, peer-reviewed awards.[24] Some members also expressed concern that important external constituencies weren’t enthusiastic about the proposal. Department of Defense grant officers, for instance, advised that an academic microfabrication facility was unnecessary because industry was the primary driver of innovation in miniaturization of electronics. Researchers at Bell Labs and at other corporate leaders in microfabrication development expressed similar views.[25] Robert Noyce maintained that an academic microfabrication facility would be worthwhile only if it were to “stick to the idea of working on the general concept of making small structures” and leave *silicon* microelectronics research to firms in Silicon Valley. Harris seems to have been responsive to that argument, since he made sure that “the relationship of submicron techniques to engineering and science concerns outside of VLSI [i.e., for making things other than silicon ICs] was emphasized in every presentation to the NSB.”[26]

To assuage the concerns of the National Science Board, Jay Harris organized three workshops to be held in May of 1975—one at the University of Pennsylvania, one at the University of Utah, and one at Washington University. Attendees were enthusiastic and repeatedly emphasized the industrial relevance and economic importance of silicon microelectronics as justification for such a center. The report from the Washington University workshop, for instance, noted that "adding to the urgency of the need for research in the submicron domain is the effort made by our international competitors to leap-frog the U.S. technology in this field. The most noteworthy program is the Japanese decision to spend $233 million in the next four years to develop submicron device research and fabrication capabilities with their industry-university teams."[27]

The concern for the needs of American industry in keeping ahead of foreign competitors was recognizably new. As the report from the Penn workshop put it,

> foreign competition [is] a subject not normally viewed as part of a NSF sponsored Workshop's concern. However, if the electrical engineering academic community is to assess its priorities for the 1980's [*sic*], the health and vigor of the American electronics industry is an essential consideration. … When one considers the obituaries of such industries both here and abroad as consumer electronics, cameras, electron microscopes and large tankers that have fallen before the intense developmental efforts that Japan has become properly and respectedly [*sic*] famous for, it raises a grim specter for this country. Other countries like Germany and France are active in submicron fabrication as well. We court serious economic danger if the United States government fails to respond with adequate resources in this new area for technological supremacy.[28]

Accordingly, attendees devoted a significant part of their time to discussing how a microfabrication center could foster university-industry cooperation.

Coincidentally, Harris' three workshops took place within weeks of the first meeting of another conference that, like the National Research and Resource Facility for Submicron Structures, would come to be one of the infrastructural institutions of microfabrication research: the Gordon Research Conference (GRC) series on the Chemistry and Physics of Microstructure Fabrication (sometimes called the Gordon Microfabrication Conferences). This series of conferences was the outcome of a proposal that Robert Keyes of IBM made to the Gordon Research Conference home office in 1975.

Recall that Robert Keyes was mentioned in chapter 2 as a leading advocate of low-temperature circuits and an influential theorist of the limits to microelectronic device scaling. We will run into him again in chapter 6 when we return to the molecular electronics field's development in the late 1980s. Keyes turns up so often in this story because—like Jay Harris or Forrest Carter—he was using institutional innovations to ameliorate network failures that he believed hindered technological innovation. Unlike Harris or Carter, of course, Keyes was acting on behalf of a firm, IBM, rather than the state (though IBM's status as a regulated quasi-monopoly blurs that distinction). In other respects, though, Keyes was using the same tools as state network builders, and to the same end: to get other actors to know and trust one another so that needed information would move more quickly through the relevant research communities.

Curiously, Keyes believed that network building was necessary in the microfabrication community because recent progress in the field had been too fast, leaving microfabrication an empirical "black art" lacking a scientific explanation or a rational basis for improvement:

> [M]odern technology depends on fabrication of intricate microstructures. … The art of fabricating such integrated structures has advanced more rapidly than the basic understanding of the processes involved. Nevertheless, a scientific foundation for the technologies will be essential to their optimization. The proposed conference is intended to bring science into contact with the art.[29]

Keyes' Gordon Microfabrication series, Carter's Molecular Electronic Device conferences, and Harris' *ad hoc* workshops may seem to be dull epiphenomena of history of science. But as Keyes' proposal implies, conferences *are* science—especially in fields in which progress depends on practitioners of one art getting to know and trust practitioners of another.

Of course, when Keyes proposed his Gordon meeting, the American microfabrication community already had its own conference series—the Three Beams meeting. By the mid 1970s, though, the Three Beams meeting had become formal and large enough that Keyes saw a need for a smaller, more *gemeinschaftlich* meeting at which specialists would share and debate methods intensively and would interact informally. The Gordon Conferences provided an ideal format for such debate and interaction. "The Gordons," begun in 1931 as small conferences in cutting-edge research areas, had grown to about ninety conferences a year by 1975.[30] Gordon Conferences were long (usually four full days, with participants expected to stay the whole time), remote (usually held at small boarding schools in rural New England), invitation-only, small (never more than 150 people, often only 30 or 40), off-the-record (no proceedings were compiled, and talks were not to be cited in the published literature), and cutting-edge (talks were to cover material never presented before). The oddest of all their characteristics was the scheduling. Two or three plenary talks filled each morning and each evening, often with copious amounts of beer available after the evening plenary sessions. In between, participants did whatever they wanted—there were no scheduled activities. For Keyes, this made the GRC format ideal for an emerging microfabrication community that needed some off-the-record way to exchange ideas and unpublished tricks for making tiny devices. That kind of communication required trust that could be built more easily during a sail on Lake Winnipesaukee or a hike through the New Hampshire woods than through formal, scheduled activities.

Each Gordon Conference is part of a series of meetings, usually held every two years, on a specialized research topic. The GRC home office monitors meetings closely, eliminating series when they became too mature and soliciting suggestions for new series from attendees. Gordon Conference administrators probably took Keyes' suggestion for a microfabrication series seriously in part because he had been vice chairman of the series on Research at High Pressure in 1970. However, Keyes had more difficulty convincing the microfabrication community that his Gordon Conference solved a problem they didn't know they had. Keyes was a device physicist, not a microfabrication specialist. Thus, as chairman of the first Gordon meeting, he needed a vice chairman who was well known to the microfabrication community and who could help him select the right speakers and get commitments from his peers. He therefore asked Ed Wolf of Hughes Research to be his vice chairman—the same Ed Wolf who in 1973 had served as program chairman of the Three Beams meeting, who had helped Jay Harris fabricate microelectronic devices for research, and who would eventually be the first non-interim director of the facility Harris funded.

The NRRFSS Competition

As might be expected, there was significant overlap between Harris' workshops of May 1976 and the first Gordon meeting, held the next month. Five of the speakers at the GRC had also been speakers at one or more of Harris' workshops, and several more organizers and speakers from Harris' workshops also attended the GRC meeting. Thus, the same issues—global competition, university-industry cooperation, and the relationship between basic and applied research—hovered over both Harris' and Keyes' meetings. As the conference monitor told the GRC home office,

> This is a field in which industrial competition currently stifles much basic research. There is no time for it because of the pressure to get devices on the market, with guesses being made as to the best procedure for a particular need. There is recognition, particularly in government circles, that empirical techniques are inadequate, and that a reserve of basic research must be built up to compete with other countries.[31]

As Keyes and Wolf both later reported, Japan's VLSI program (as a demonstration of the shortcomings of the *laissez-faire* American approach to microfabrication research) and the NSF's national submicron facility (as a possible solution to those shortcomings) loomed large in the informal discussions at that first Gordon conference.[32]

In fact, the Gordon conference provided an opportunity for researchers to prepare for the submicron facility competition that they knew the NSF would soon announce. Harris' boss, Charles Polk, attended the GRC meeting, and Keyes' list of speakers contained most of the people who made a serious run at winning the NSF competition—notably, Tom Everhart (who led the Berkeley proposal with help from Ed Wolf) and Henry Smith from Lincoln Lab (who led the proposal for a facility "under M.I.T. management in Lexington, Massachusetts, adjacent to M.I.T. Lincoln Laboratory").[33] Surprisingly, though, none of Keyes' speakers were affiliated with Cornell University, and only one GRC attendee was—even though Cornell eventually won the NSF competition.

How, then, did Cornell win? From the remaining documentary record, all we can do is infer the NSF's thinking. We know that seventeen proposals were submitted by February of 1977, and we know that that list was whittled down to three leading contenders: Cornell, MIT/Lincoln Lab, and UC Berkeley. Berkeley was the leader in electron-beam lithography, while Lincoln Labs' Hank Smith was the acknowledged pioneer in x-ray lithography.[34] Cornell had an edge in none of the "three beams," although its proposal claimed some capacity in both x-ray and e-beam lithography.[35]

The NSF, however, was looking for more than local expertise in lithography—which, as Cornell later showed, could be built up quickly through strategic hiring. Instead, the NSF wanted a particular culture that would promote a high degree of integration between the proposed facility and other centers located on the same campus. In that respect, Cornell's Materials Science Center probably boosted its bid over the line. As one of the first three Materials Research Laboratories, the MSC was an important organizational model for the other MRLs and for center-based funding within the NSF—to the extent that Hyungsub Choi has aptly dubbed it the *primus inter pares* of the MRLs.[36] In writing their proposal for the NRRFSS, therefore, members of the Cornell team banked heavily on the MSC. "We have the strongest MRL with excellent supporting facilities (thought to be important at [the Penn] Workshop)," they wrote to their dean, and "We are one of two universities in the country where significant research on E-beam lithography has been done. (The other is Berkeley and they have no MRL)."[37] MIT did have an MRL, the Center for Materials Science and Engineering, but Smith gave it little mention in his proposal other than to note that users could travel from Lincoln Lab to MIT's main campus (a distance of 13 miles) to use the center's electron microscopes.

Having an MRL also helped Cornell because it implied a record of the type of interdisciplinary collaboration that was thought necessary for microfabrication research. As Keyes put it in his GRC proposal, "Microstructure fabrication involves an extreme spectrum of disciplines: Physics, many branches of chemistry (photochemistry, electrochemistry, surface chemistry) and highly technical specialities [*sic*] such as optics and electron microscopy."[38] Thus, in their NRRFSS proposal the Cornell team pointedly noted that "a very significant feature of [Cornell's] MRL is that it has provided an atmosphere of cooperation among faculty members in different departments [including] electrical engineering, materials science, applied physics, physics and chemistry."[39] Even Tom Everhart later told Cornell's dean of engineering that he "perceives a constructive attitude toward interdisciplinary work at Cornell, while at Berkeley such interaction is more difficult to achieve."[40] Similarly, although Hank Smith's NRRFSS proposal forecast several interdisciplinary applications of microfabrication (most of which eventually came to pass) in astronomy, in alternative energy (fusion and solar) research, and in biology, these were only projections, not examples taken from a long institutional history.

Finally, the Materials Science Center aided Cornell's NRRFSS proposal by showing evidence of the kinds of university-industry interactions that the NSF was eager to promote. As the members of the Cornell team told their dean, "we have a history of successful collaboration with industry in our semiconductor work. ([Harris' Penn] Workshop felt industrial participation was important.)"[41] Other proposals tried to hit a similar note, with varying success. For instance, a proposal by the University of Pennsylvania, Drexel University, and Lehigh University played up those universities' proximity to (and somewhat lukewarm letters of endorsement from) such major corporate R&D players as IBM, Sperry-Univac, RCA, and Bell Labs, as well as "a small silicon house, MOS Technology of Valley Forge."[42] Presumably UC Berkeley and MIT's records of university-industry linkages were among the reasons their proposals joined Cornell's on the short list. UC Berkeley's Department of Electrical Engineering certainly could boast of deep ties with industry, and Hank Smith could point to a number of successful technology transfers from Lincoln Lab's microfabrication research and thereby plausibly claim that his facility's "intended purposes [would be] the development of submicrometer technology and the transfer of that technology to universities and commercial firms."[43]

Stanford, the Dog That Didn't Bark in the Night

Whatever the reasons, Cornell slipped past UC Berkeley and (more narrowly) MIT. Cornell's victory then precipitated several dramatic moves within the upper echelons of the microfabrication community. Tom Everhart attempted to recruit Hank Smith to Berkeley with this offer: "You come out here as a faculty member. I'll raise the money, you do the work. We'll set up our own nanofabrication facility and we'll beat the pants off of Cornell."[44] As a counteroffer, the director of Smith's division at Lincoln Lab "asked what I [Smith] was going to do. So I told him that I would like to demonstrate that the NSF had made a big mistake. He says, 'Great! Let's do it.' Just like that, they gave me a million dollars. A million dollar budget, where in the hell did that came from? I didn't know there was that much fat in the budget."[45] Lincoln Lab's Submicron Technology Program would be "operational by late 1977."[46] Meanwhile, MIT recruited Smith to take an adjunct position and build a Submicrometer Structures Laboratory on the main campus. That facility opened in 1978, even before Cornell's NRRFSS did.[47] In 1980, Smith took a full-time faculty position at MIT.

At Cornell, Joseph Ballantyne, a professor of electrical engineering who led the proposal team, became interim director of the facility but immediately began recruiting a permanent director with more stature in the microfabrication community. His short list consisted of Fabian Pease, Ed Wolf, Tom Everhart, and Truman Blocker from Texas Instruments.[48] Wolf was hired, perhaps because (as Jay Harris' experience showed) he was accustomed to dealing with outside users while at Hughes. Everhart, however, made enough of an impression that he was offered the position of dean of Cornell's College of Engineering instead. As dean, his responsibilities included working with Wolf to get the facility running.

Pease, meanwhile, was also being recruited by Stanford, the most prominent university not to have entered the NRRFSS competition. As we saw in chapter 1, Stanford already had a microfabrication facility—the Integrated Circuits Laboratory—that outpaced anything at MIT or Cornell. Stanford also had a Materials Research Laboratory, and a history of interdisciplinary collaboration at least as deep as MIT's or Cornell's. Moreover, unlike MIT or Cornell, Stanford had a thriving semiconductor-manufacturing district nearby. That is, all of the factors that led to the short-listing of Cornell and MIT would have been more advantageous for Stanford had it competed. Even without competing, Stanford shaped the outcome. For instance, Berkeley's bid was weakened by the presence of the Integrated Circuits Laboratory on the other side of San Francisco Bay. Stanford's proven microfabrication capacity also helped define the National Science Foundation's criteria for what a national facility should be able to do. After all, when James Meindl spoke at Jay Harris' University of Utah workshop, his recommendation that a national user facility be capable of fast turn-around on a wide variety of circuits and be able to accept remote orders for mask designs from non-local users carried considerable weight, as the ICL was already doing "150 to 200 different mask designs per year."[49]

Then why didn't Stanford compete? One factor was probably that the NSF was still a new and marginal funder of the engineering sciences. Meindl had reason to be skeptical that the NSF would be able to sustain support for a microfabrication facility over the long term. Meindl may also have believed that catering to the customers of a national user facility would leave little time for his other pursuits. By the time Cornell and MIT founded their microfabrication facilities, though, Meindl and John Linvill had concluded that they would have to respond. They hatched a plan for a "Center for Integrated Systems" (CIS) that would link together most of the Stanford faculty members whose research touched on advanced circuits and computing. The CIS would interweave the activities of four pre-existing laboratories—in ascending order of scale, the Solid State Electronics Lab, the Integrated Circuits Laboratory, the Computer Systems Lab, and the Information Systems Lab.

To accomplish the aim of "integration," Linvill and Meindl argued, they would need a new building in the heart of Stanford's expensive real estate to house the ICL's tools and clean rooms, offices for faculty members and for researchers from both the Electrical Engineering Department and the Computer Science Department, and gathering spaces designed to promote interdisciplinary interaction.[50] Before, the ICL's tools had taken up half the basement of McCullough Hall, a building designed for Stanford's Materials Science Center. By the mid 1970s, though, microfabrication techniques had become so sensitive that they demanded their own built environment. Older buildings, built for a different purpose, were susceptible to stray sounds (and other vibrations), to light (and other electromagnetic radiation), to magnetic fields, and to dust, all of which interfere with the ability to fabricate microelectronic devices. The radiation, vibration, and dust associated with McCullough Hall hadn't been insurmountable problems when the smallest integrated circuit features were approximately a micron in size. But once again Moore's Law intervened. By the late 1970s the smallest circuit features were below a micron in size, and getting smaller every year. The McCullough basement was an unsuitable environment for fabricating circuits at that scale.

At Cornell, Ed Wolf lobbied for a new building for his NRRFSS, and for the same reasons. As the newsletter of the NRRFSS user program explained,

> The building itself has presented a challenge. Construction was delayed about a year as Amman and Whitney, Inc., conducted extensive studies of the vibration environment, and IIT investigated electromagnetic interference at the site. In accordance with the vibration study, equipment sensitive to vibration will be placed on heavy concrete slabs isolated from the building on a special bedding in the earth. Specifications for construction materials and the location of electrical wiring were made so as to minimize electromagnetic interference in areas that house electron-beam equipment.[51]

Clean rooms are much more expensive than ordinary laboratory buildings, though. In raising money for their new microfabrication facilities, MIT, Cornell, and Stanford all encountered a harsh reality of post-1970 American science policy: The federal government was no longer as willing to pay for bricks and mortar as it had been during the early Cold War. Whereas in the early 1960s all three schools had received millions of dollars from ARPA to pay for new buildings for their materials research laboratories, in the late 1970s and the early 1980s the NSF was willing to pay for tools, research, and operations, but not for new microfabrication buildings.[52]

The sources of the private donations needed for these new buildings indicate important differences among the three schools, particularly between Cornell and Stanford. At Cornell, funds came largely from private philanthropies, especially the Pew Charitable Trust, and from an alumnus, Lester Knight, for whom the building was named.[53] At Stanford, Linvill and Meindl carefully cultivated an elite group of American companies to form a CIS "industrial affiliates" program. With the help of John Young, director of research at Hewlett-Packard, and George Pake, director of Xerox's famed Palo Alto Research Center (PARC), Linvill and Meindl succeeded in persuading nineteen firms to offer an initial $750,000 (most spread the sum over three years), then $100,000 per year for the construction and operation of the CIS.[54] In chapter 5 we will examine what these firms expected to get—and did get—in return.

Creating an Organizational Field

Linvill and Meindl were also eager to have federal support for the research that would be conducted in their new facility. Thus, in the late summer and fall of 1978 they struck up a vigorous correspondence with Jay Harris and his supervisors (and their supervisors), all the way up to James Krumhansl, the NSF's Assistant Director for Mathematical and Physical Sciences and Engineering.[55] Stanford's lobbying was well timed. Krumhansl was just starting an NSF program in Microstructures Science, Engineering, and Technology (abbreviated μSET). Krumhansl seems to have intended this program to foster a new proto-discipline focused on microstructures in the same way ARPA had built its network of MRLs to foster a new discipline of materials science in the early 1960s. To that end, Krumhansl encouraged publicity for, and linkages among, the new wave of academic microfabrication facilities so that they could serve as models to be copied at other universities.

For instance, Krumhansl was the "moving force" behind a November 1978 workshop organized by the Cornell facility and steered jointly by the National Science Foundation and a National Research Council panel on Thin Film Microstructure Science and Technology.[56] The membership of that panel gives a sense of the connections among the new research fields and institutions that tied themselves to the semiconductor industry in the 1970s: Everhart, Meindl, and Alan Heeger were on the panel, and the panel's four advisers were Hank Smith, Ed Wolf, Carver Mead, and Gordon Moore; Moore and James Meindl were also on the board of the NRRFSS. Moreover, the panel's recommendations show that those connections among corporate, academic, and government actors arose in the late 1970s because the perceived weakness of the US semiconductor industry was seen as a threat to *both* economic and national security:

> The United States has led in the development and exploitation of modern solid-state electronics technology; whether it will maintain this leadership is by no means certain. … Japanese industry, with active and extensive support from the Japanese Government, has mounted an intense research and development effort in microfabrication. … Significant research and development efforts are under way in England, Holland, France, and West Germany. In addition, the technologies employed in national defense depend on semiconductor electronics; therefore, leadership in semiconductor electronics is essential to our national security.[57]

Not surprisingly, a panel dominated by scientists concluded that meeting the needs of national and economic security would require "a new and expanded set of coordinated research programs in microstructure science and engineering," including "Regional Research Centers … comparable in scope with the Materials Research Laboratories and the Cornell Submicron Facility."[58]

As the connections among different organizations and research fields represented on the NRC panel implies, Krumhansl was following the path of Harris, Keyes, Carter, and other institutional innovators in using conferences, committees, and professional societies to overcome a network failure—in this case, the failure of microstructure science and engineering to be "recognized as constituting a discipline … [and] lack of support for … problems that fall between the boundaries of the traditional disciplines."[59] Krumhansl also reached for another classic tool for repairing network failure: the para-scientific media. In November of 1979, *Physics Today* published a special issue on "microscience" that featured a piece by Wolf describing the NRRFSS, contributions from IBM and Hewlett-Packard, and an overview by Krumhansl and Yoh-Han Pao.[60]

Krumhansl and Pao's article has since become rather famous in the history of nanotechnology because it was there that Eric Drexler first heard of Richard Feynman's 1959 "Room at the Bottom" speech.[61] But in fact the influence of Krumhansl's vision for microscience extended well beyond Drexler. Ordinary scientists read the *Physics Today* article and incorporated it into their grant proposals for a new kind of research defined by size (the microscale) rather than by subject matter. Many of the participants in Krumhansl's workshop and the NRC Microstructure panel led institutions that would later become central nodes of nanoscience—a field similarly defined by size rather than by subject.[62] It is hard to draw a straight line between Krumhansl's μSET and today's National Nanotechnology Initiative, but indirect resonances between them are abundant.

Much of what Krumhansl and the NRC's Microstructure panel recommended came to pass, some of it in the short term. In particular, microfabrication research centers proliferated in the early 1980s; the Cornell, MIT, and Stanford facilities soon emerged as leaders to which other facilities compared themselves. Among those other facilities were Caltech's Silicon Structures Project (begun in 1977), the University of Minnesota's Microelectronics and Information Science Center (opened in 1980), Rensselaer Polytechnic's Center for Integrated Electronics (opened in 1981) and Arizona State's Center for Solid State Electronics Research (opened in 1981).[63] By the mid 1980s, at least one or two campus facilities opened every year. For example, there was one at the Rochester Institute of Technology (opened in 1985), one at the University of Michigan (opened in 1986), one at Yale (opened in 1988), and one at the University of Cincinnati (opened in 1988).

In some cases, institutional entrepreneurs within universities used the founding of other institutions' microfabrication facilities as an argument for resources to expand pre-existing rudimentary efforts. For instance, the University of Arkansas "obtained its first fabrication facilities in the late 1960's … [but] in 1978, largely through the efforts of Dr. W.D. Brown … the [Electrical Engineering] department obtained considerable additional equipment through grants from Sandia Laboratories, Texas Instruments, and the National Science Foundation."[64] Similarly, "development of the Auburn University Microelectronics Laboratory began in 1975," but it expanded considerably when "the Alabama Microelectronics Science and Technology Center (AMSTEC) was formed at Auburn University in 1984, following a special legislative appropriation of $250,000/year."[65] These new microelectronics and microfabrication facilities almost always cited their predecessors as both models and competitors. As the proposal for a Microelectronics Research Center at the University of Texas put it in 1983,

> The economy of the State of Texas is rapidly moving toward high-technology industries, particularly in microelectronics and computers. … The purpose of this proposal is to insure that the University of Texas is the leader in that effort. Development of microelectronics research centers has begun at a number of universities (Table 1) as a response to the widely perceived necessity for fundamental and applied work in these areas.[66]

Table 1 then listed, in order, data on the facilities at Cornell, Stanford, MIT, North Carolina, Arizona State, and Minnesota.

For the leading microfabrication facilities, there were real benefits to fostering peer facilities. An MIT faculty member reported the following to the Submicrometer Structures Laboratory team after a visit to Stanford in 1977:

> While it may seem strange to us, Jim Meindl said that he thought MIT's entry into the IC field would legitimize it, and give more emphasis to Stanford's program. I cannot overemphasize that everyone I met was most cordial and friendly, and eager to cooperate.[67]

At Cornell, leaders of the National Submicron Facility loudly advertised that they were a national resource not just for tools but also for knowledge of how to establish and operate similar facilities. That knowledge, they asserted, aided the propagation of the microfabrication organizational field through industry and academia, and helped maintain ties among the organizations in that field. As Joe Ballantyne reported in 1986, for instance,

> GE had an engineer in residence at NRRFSS for a year to learn MBE, electron beam lithography and device processing for high speed GaAs devices. He then returned to GE and established a similar processing capability. … Strong interaction continues between GE and Cornell. NRRFSS is continually called on to help/advise other companies and universities in setting up similar laboratories, such as Varian, GE, McDonnell Douglas, the Jet Propulsion Lab, Hughes, Caltech, University of Michigan and University of California San Diego. Over the last several years we have advised more than forty organizations.[68]

The NRRFSS provided advice on how to copy its model, in part because that was its mandate from the NSF. According to the NRRFSS policy board in 1986,

> A strong recommendation came out of the [NSF] site review team that the facility host a meeting of microelectronics-related center directors to encourage collaborations and technology transfer. The NSF … has endorsed this concept and will both request the facility to do so and will provide funding for such a meeting. Coincidentally a Professor Marc Heritage from the University of Utah visited submicron the day after our site visit to discuss how to establish a similar center at the University of Utah. He was very interested in a center director's meeting as he had found it very difficult to even find out what centers exist in this field.[69]

As the board's reference to "technology transfer" suggests, one point of institutional innovation on which these centers most eagerly copied and competed with one another was their university-industry partnerships.

In fact, almost all of the academic facilities mentioned above had some industrial partnership program, though these programs took many different forms: sharing facilities with industry, industrial internships for students, annual corporate "membership" fees in return for previews of faculty research, and so on. Each facility refined the form of its industrial partnership partly on the basis of intelligence about the other facilities obtained in various ways—for example, by inviting the directors of the other facilities to give presentations on their programs,[70] by making phone calls to ask industry leaders how their companies had been approached by the rival facilities,[71] by obtaining prospectuses for competitors' industrial programs,[72] or by obtaining policy memos describing competing schools' intellectual property policies.[73]

User facilities didn't always have to rely on back channels to acquire information about their peer or competitor facilities, though. In the early 1980s, Cornell's facility, in particular, was adept at drawing attention from journalists who heralded it as a harbinger of a new orientation to industry in American universities. As of early 1982, the NRRFSS (by then more widely known as the National Submicron Facility) had appeared in *Popular Science*, *Newsweek*, *Hispanic Engineer*, *Barron's*, *National Geographic*, *Industrial Research and Development*, *Chemical & Engineering News*, *Chronicle of Higher Education*, *Electronic News*, *Science*, and other publications—some "22 magazines and 43 newspapers" in all by 1985—and in televised segments on the BBC and CBS.[74] The Submicron Facility also received attention from politicians interested in applying its institutional innovations across the landscape of American science. For instance, in 1979 the House of Representatives' Subcommittee on Science, Research, and Technology summoned Ed Wolf and Richard Atkinson, director of the NSF, to testify on "Government and Innovation: University-Industry Relations."[75] Five years later, Cornell's president, Frank Rhodes, was also called before the House; in his testimony, he pointed to the National Submicron Facility as an example of how to overcome the difficulty of gaining access to increasingly expensive instrumentation.[76]

Stumbling out of the Gate

Behind all the favorable press attention, however, the Submicron Facility's actual relations with industry, and with non-Cornell users, were halting at first. As Tom Everhart noted in 1981, there was "considerable difference between the expectations of the scientific community of the Submicron Facility and what was proposed to the National Science Foundation by Cornell."[77] That "considerable difference" in expectations was, in part, simply a product of the learning curve in getting a new organization started: hiring faculty and staff, forming committees, and experimenting with administrative procedures (such as deciding what equipment to buy, what to charge Cornell and non-Cornell users for time on that equipment, and how to make decisions about what to buy and charge). Those tasks, plus designing and raising funds for the Knight Laboratory building, left Everhart and Wolf little time to recruit outside users and industrial collaborators.

The Submicron Facility's early difficulties weren't just growing pains, though; they were also evidence of ambivalence and misunderstandings about the facility's mission. Remember that the facility's original name—much maligned by almost everyone involved—was National *Research and Resource* Facility for Submicron Structures. To many at Cornell, that name implied that a large share of the NSF funding should go toward Cornell faculty members' own research, at the expense of the "Resource" mission of providing equipment for use by external visitors. That, after all, had been the model in the Materials Research Laboratories. Since Cornell had won the NRRFSS competition in part by pointing to the model of the MRLs, some Cornell faculty members believed the NRRFSS should operate on similar lines.

Cornell's hires in the years immediately after it won the NRRFSS competition indicate the importance initially placed on the "Research" mission. In particular, Michael Isaacson's arrival from the University of Chicago in 1980 catapulted Cornell from a distant follower to a leader in pushing the outer limits of microfabrication. A specialist in scanning transmission electron microscopy (STEM), Isaacson was a graduate student in Albert Crewe's lab at Chicago when Crewe's team published the first atomic-resolution STEM images.[78] By the time he arrived at Cornell, Isaacson was adapting the STEM to manipulate small clusters of atoms to form nanoscale structures—an achievement in the "research" mode that the NRRFSS trumpeted in many of its early publications. Isaacson's STEM was not, however, a national "resource" that could be rented by outside users, since untrained operators would almost certainly not have gotten good results and probably would have broken it.

In developing new techniques for ultra-high-resolution lithography, Isaacson and Cornell were moving in parallel with their peers at MIT and Stanford. In fact, Isaacson and Fabian Pease (of Stanford) embarked on a mostly good-natured race to use electron-beam lithography to write a page of text at 1/25,000 scale—the criterion for a prize of $1,000 offered by Richard Feynman in his 1959 "Room at the Bottom" speech. Isaacson's team appears to have been the first to solicit Feynman to award them the prize, but reportedly Feynman's first bout with cancer meant he was too occupied to reply.[79] In 1985, Pease and a graduate student, Tom Newman, sent Feynman a micrograph of the first page of *A Tale of Two Cities* at the appropriate magnification and were duly awarded the $1,000 (see figure 4.2).[80]

At MIT, Smith and his colleagues were also pushing the limits of resolution, though with x-ray lithography and interferometric lithography (i.e., creating a standing wave pattern in a photoresist by superimposing more than one lithographic beam at the same time) instead of electron beams.[81] However, Smith (like Pease) could focus on a "research" mission for the Submicron Structures Laboratory without having to satisfy demands from the NSF. Cornell's NRRFSS, on the other hand, was continually steered away from "Research" and toward the "Resource" mission. As Smith puts it,

> I always considered myself kind of fortunate that I didn't get [the NRRFSS]. Because I was able to do better and more innovative things here than if I was director of some lab like the Cornell Lab. At Cornell, they really don't do any research—they perform a service. … What they do is buy commercial equipment and make themselves available for commercial processing.[82]

Conversely, some at Cornell may have regretted winning the NRRFSS once they realized that the NSF wanted them to perform a "service" rather than do cutting-edge research. As Ed Wolf later reminisced,

> It took several years to create the funding and the "national" user concept required to wrest successfully several key instruments from the control of a few faculty members who believed in their programs first, other Cornell faculty next,

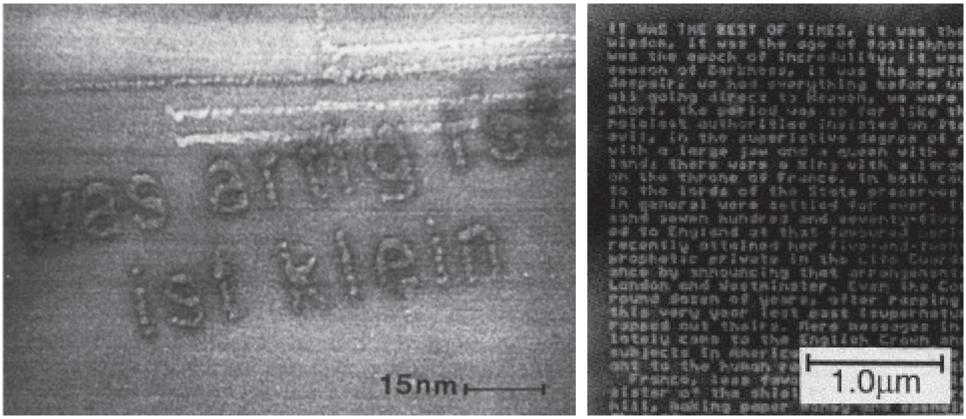


Figure 4.2
Electron-beam lithography “literature”: at left, the quotation from Lessing produced by Michael Isaacson’s Cornell team. The quotation means “whatever is well-behaved, is small.” At right, the first page of *A Tale of Two Cities* produced by Fabian Pease’s Stanford team. The image at left is reprinted with permission from M. Isaacson and A. Murray, “*In situ* Vaporization of Very Low Molecular Weight Resists Using ½ nm Diameter Electron Beams,” *Journal of Vacuum Science and Technology* 19 (1981): 1117–1120, copyright 1981 AIP Publishing LLC. The image at right is reprinted with permission from T. H. Newman, K. E. Williams, and R. F. W. Pease, “High Resolution Patterning System with a Single Bore Objective Lens,” *Journal of Vacuum Science and Technology B* 5 (1987): 88–91, copyright 1987 American Vacuum Society.

> and then “outside” users. I will quickly add that NSF and the outside user community had just the reverse order of priority. This was not a stress-free time for the director of NRRFSS. I have a thick file and little remaining hair to attest to this fact.[83]

At the end of its first five-year grant, therefore, the Cornell facility faced considerable criticism for failing to be a “national” resource. Indeed, the National Science Board made its authorization for renewal of the facility’s grant dependent on submission of more detailed reports on (and greater commitment to) its user program.[84] Accordingly, the facility hired more full-time employees to help users and named a younger member of the faculty, Robert Buhrman, Associate Director for the User Program.[85] Under Buhrman’s leadership, the user program quickly took off, and by 1986 “over 60% of the normal 40 hour work week at the facility was utilized by non-Cornell user research projects.”[86] Most of those non-local users were from other universities; as of 1985, however, 12 percent of users came from industry and 7 percent from government labs.[87] As Joe Ballantyne reported in 1986, the facility's industrial affiliates

> rank our program as equivalent to or better than those at Illinois, Stanford and MIT. No one has yet told me that another school does a better job of industrial interactions than Cornell. We have active collaborative research with industry directly in the facility (e.g. AT&T Bell Laboratories, McDonnell Douglas and General Electric) and the largest source of support for the projects utilizing NRRFSS is industrial grants/contracts (industry 33%, NSF 26%, and DoD 24%, others 17%).[88]

Centers Become a Way of Life

The aforementioned ties to industry proved necessary not just for generating funding and collaborative research, but also for recruiting a vocal constituency to speak on the facility's behalf. That can be seen most clearly in Cornell's fight with the NSF's director, Erich Bloch, at the end of its second five-year grant in the mid 1980s. Facing an imminent loss of funding, Cornell called on its industrial contacts to lobby the NSF. The industrial representatives on the facility's policy committee—including Gordon Moore and executives from IBM, Bell Labs, GE, and Motorola—dutifully co-signed a letter to Nam Suh, head of the NSF Engineering Directorate.[89] Executives from the facility's industrial affiliates, including Xerox and Raytheon, also sent letters of support, as did collaborators from government laboratories and universities. Under the weight of that response, Bloch and Suh ultimately relented.

Bloch's opposition to the National Submicron Facility stemmed in part from an earlier clash with Wolf that had left him believing that Cornell was not sufficiently in line with the Reagan administration's promotion of university-industry ties. In 1980, when Bloch was still an IBM vice president, IBM approached Stanford and then Cornell with an offer to donate a used electron-beam lithography system, which both schools declined. Once he became the NSF's director, "every time NRRFSS was discussed with Mr. Bloch he brought the issue [of the spurned e-beam lithography system] up as evidence that Cornell is not serious about getting industrial support for NRRFSS."[90] Bloch may well have drawn an invidious distinction between Cornell and Stanford, since he heaped praise on Stanford's Center for Integrated Systems as "a good illustration of the linkage between scientific research and industrial competitiveness that national policymakers are attempting to foster in universities."[91] I have found no indication that Bloch (unlike his predecessors) spoke similarly of Cornell's Submicron Facility.

Paradoxically, though, the organizational model forged at Cornell contributed greatly to Bloch's own legacy as NSF director. Bloch had been installed at the NSF largely to carry out the Reagan administration's agenda of gearing American academic science more closely to the needs of high-tech industry. The vehicle for that vision that Bloch pushed through in 1984 was the Engineering Research Centers program—a suite of academic facilities (originally six, then thirteen as of 2013 plus twenty-nine "graduated" facilities) that, as Dian Belanger puts it, "addressed immediate concerns in both engineering research and engineering education—concerns articulated by both academe and industry."[92]

In Belanger's view, the ERCs "represented a fundamental rethinking of traditional NSF engineering activity." Over time, they came to be seen as such a successful program that they spawned cascades of new center programs at the NSF, including the Science and Technology Centers and the Centers for Research Excellence in Science and Technology programs in 1987, the Materials Research Science and Engineering Centers (a revamped version of the MRLs) in 1994, the Nanoscale Science and Engineering Centers in 2001, and several smaller center programs (e.g., Centers for Analysis and Synthesis, Centers for Chemical Innovation, and Science of Learning Centers)—not to mention other interdisciplinary centers that weren't part of any larger center program. Interdisciplinary centers—and especially programs spawning peer groups of centers—have become an almost instinctive mode of funding at the NSF, and of doing business on American campuses.

The NRRFSS was an important step toward Bloch's and the NSF's adoption of center programs. Indeed, Gordon Moore and the other industry representatives on the NRRFSS' Policy Board explicitly argued, when lobbying Bloch and Suh not to pull Cornell's funding, that "the National Science Foundation's investment in the Submicron Facility has enabled it to serve as a model for scientific and engineering centers nationally."[93] Suh, in turn, acknowledged that "the model of NRRFSS as a user facility role has been utilized in the planning and establishment of NSF's Regional Instrumental Laboratory program. More recently, the interdisciplinary operation of NRRFSS has provided the feasibility model for the innovative ERC program."[94]

Looking back in 1999, Joe Ballantyne made an even more expansive claim that Cornell University and the Submicron Facility he had founded had helped make academic interdisciplinary centers programs an indispensable tool of science policy:

> During the mid- to late ’80s, Cornell enjoyed remarkable success in establishing interdisciplinary research centers. Towards the end of that decade, Cornell was the number one recipient of NSF dollars and the number two U.S. Research University as measured by research expenditures. Perhaps for this reason, the National Science Board used the Cornell approach to centers as its model for a greatly expanded University Research Centers program. The Science Board held two meetings at Cornell, and took testimony from Cornell in at least two of its meetings in Washington. Subsequently, DARPA used the model in several of its University Research Center programs.[95]

Yet Cornell’s success with interdisciplinary centers was in some ways a double-edged sword. For some members of the science-policy elite, such as Bloch, Cornell had, by the mid 1980s, become too big to fund any further. For others—such as Gordon Moore and the rest of the Submicron Facility’s advisory board—the Submicron Facility was by then too big to fail, since cutting off funding would leave its user base stranded.

By 1985, then, the ball that Jay Harris started rolling in 1974 had reached a fork in the road. University microfabrication facilities were now commonplace, and academic microfabrication research was squarely on the agenda of science policy. Perhaps the NSF had accomplished its goals for the NRRFSS and now could redistribute microfabrication funding more widely. The Submicron Facility’s funding portfolio had become diverse enough (in its first nine years the facility brought in “$25 million from NSF, $7 million from the university and private sources, $14 million from other federal agencies, and $31 million from industry”) that retrenchment by the NSF would not have been unthinkable.[96] Yet the Cornell team argued that a “national” facility, heavily funded by the NSF, was still necessary.

Thirty years later, the question of whether a “national” facility is needed still hadn’t been resolved. As we will see in chapter 5, each time the question was raised, a temporary answer emerged that reflected—but also inflected—the prevailing science policy paradigm. A continuous thread through all those answers, though, is that each time Cornell’s Submicron Facility reinvented itself, it did so in a way that pushed itself, Cornell, and the NSF closer to accepting nanotechnology as an organizing principle for science and science policy. That was true even in 1985, when few people had heard of nanotechnology. By rebranding from “micro-” or “submicron” to “nano”—i.e., rebranding from “millionths” or “sub-millionths” to “billionths” of a meter—Cornell’s facility and its peers staked a powerful claim for organizational progress. That is, the name change itself argued that these facilities had attached themselves to Moore's Law and were inexorably marching down the size scale in step with industry.

Cornell was among the first universities to try that gambit. In 1985 it submitted a proposal for the first round of NSF Engineering Research Centers that would have reinvented the Submicron Facility as the "Cornell Nanoelectronics Engineering Center"—an interdisciplinary center in the "Research" rather than the "Resource" mode.[97] Perhaps not surprisingly in view of the NSF's earlier insistence that the facility fulfill its "resource" mission, that proposal was bluntly turned down. With the support of its industrial and academic partners, though, the Submicron Facility's leaders were able to put enough pressure on the NSF the next year to get it to accept a new, non-competitive proposal that dedicated the organization almost exclusively to serving external users. Along with that narrowing of mission came a new name, which it would keep until 1993: the National Nanofabrication Facility.

Today, Cornell's flirtation with turning the Submicron Facility into a Nanoelectronics Engineering Center is barely remembered, partly because the facility model of providing shared tools to local and visiting users has proved remarkably durable and partly because the Submicron Facility's descendants have forged wide-ranging interdisciplinary connections, especially with the life sciences, that make an exclusive focus on electronics seem parochial. I have argued elsewhere that the shift (both in rhetoric and technology) from "submicron" to "nano" was an important tool in making microfabrication a more interdisciplinary field.[98] Those interdisciplinary connections will be an important theme when we pick up the story of the Cornell and Stanford facilities again in chapter 5.

Yet it is important to remember that a primary reason the Submicron Facility was founded was to aid the domestic *microelectronics* industry in a moment of national crisis. That goal remained front and center in the Submicron Facility's recruitment of faculty members and advisory board members; in the research collaborations with IBM, Bell Labs, and other microelectronics firms that its faculty members undertook; in Cornell's use of the Facility in the mid 1980s to acquire a Semiconductor Research Corporation-funded microelectronics research center and other large grants; and in the Facility's fight with the NSF to maintain funding. Moreover, the real and perceived ties between the Cornell, Stanford, and MIT facilities and the domestic semiconductor industry were a major factor in the founding of similar facilities on campuses all over the country throughout the 1980s and the 1990s.

Finally, in a turn of events that was most indicative of the "long arm of Moore's Law," the Submicron Facility, the Submicrometer Structures Laboratory, and the Center for Integrated Systems forged partnerships with the microelectronics industry that were widely attended to by policy makers, by journalists, by executives, and by university administrators and faculty members in the mid 1980s. The model established by those facilities was then folded into a variety of efforts to foster university-industry partnerships across a wide range of industries beyond microelectronics. Most notably, the Submicron Facility and its peers were gestured to as partial models for the NSF's Engineering Research Centers program and succeeding NSF center and facilities programs and for center programs of other federal agencies and independent interdisciplinary academic research centers and facilities at universities throughout the United States and around the world. What began in microelectronics is now commonplace for almost all academic endeavors that have, or want, some connection to high-tech industry in general.

5 Circuits, Cells, and Networks

As we have seen, the American semiconductor industry began the 1980s in a panic over the rapid gains made in the previous decade by overseas competitors, particularly in Japan. Federal policy makers shared that panic because of the economic importance *and* the national-security importance of the domestic semiconductor industry. As the White House Science Council Panel on Semiconductors put it in 1987,

> Semiconductor production and design technologies in use in the U.S. commercial sector flow into military production, rather than the reverse. Thus, maintaining a state-of-the-art industrial capability is a legitimate national security concern. … While our dependency on foreign sources is modest today, semiconductor manufacturing trends indicate that we will become highly dependent on foreign sources sooner rather than later. … U.S. semiconductor technology leadership is rapidly eroding and that this has serious implications for national security.[1]

Those “implications for national security” were especially persuasive in the waning days of the Cold War. For instance, one frequently invoked scenario had Soviet submarines disrupting transoceanic shipping at the start of a third world war and thereby denying the US military the chips it needed to win on the battlefield.

Yet it was not “dependency on [generic] foreign sources” that spurred US policy makers to bolster domestic semiconductor manufacturers so much as competition with specifically Japanese firms. After all, Congress and the executive branch did little to limit US firms’ transplantation of their own manufacturing to other countries, and they expressed little concern about the nascent electronics sectors in South Korea or Taiwan or the purchase of major US semiconductor manufacturers by European conglomerates (Signetics by Philips in 1975, Fairchild by Schlumberger in 1979). When it came to competition with Japan, though, US policy makers and leaders of the US semiconductor industry called for quick action.

For instance, in 1978 National Semiconductor put out a pamphlet, titled The Japanese Threat: Courteous Destruction, warning that Japanese firms had "overrun" one US industry after another, and that semiconductors would be next:

> Japanese companies, operating in the form of government supported groups, intend to dominate the US semiconductor industry just as similar efforts have overtaken many other American industries. Unless the Federal Government … enforces free-trade rules for Japanese-made products now, the semiconductor industry here will be overrun and destroyed within ten years. Successful domination of markets by Japanese companies has weakened or destroyed … such [US] industries as steel, motorcycles, CB radios, television receivers, high-fidelity electronics, sewing machines, calculators, and various passive electronic components. These industries … are now dominated by Japanese companies which gained control through predatory pricing.[2]

Incendiary rhetoric of this sort was intended to provoke action from the federal government that would match that taken by MITI and other agencies in Japan. Indeed, we have seen that MITI programs in VLSI semiconductor integrated circuits, superconducting computing, and molecular electronics were useful ammunition to institutional entrepreneurs trying to get the federal government to support organizational experiments such as the National Submicron Facility.

The best-known US organizational experiments that emerged in response to MITI and the growing competitiveness of Japan's semiconductor industry were a series of industrial research consortia designed so that firms could share both the burdens and the benefits of investing in research and development. At the same time, American universities began to experiment with academic microelectronics research centers working closely with industrial partners. There is already a significant body of scholarship analyzing both of these developments, and in this chapter I will draw on that literature without recapitulating it.[3] In this chapter I go beyond earlier work in delineating the multiple, complex connections between industrial consortia and university-industry partnerships in the semiconductor field. I argue that many academic microelectronics research centers acted *as* industrial consortia (at least in part), that industrial consortia benefited from the existence of academic microelectronics research centers and vice versa, and that therefore these centers were commingled and symbiotically co-evolving organizational forms.

At least by the metric of the ratio of the American and Japanese shares in the global semiconductor market, these organizational experiments came to be counted as successes. In the late 1980s, Japanese semiconductor firms began to lose global market share, and by the late 1990s American firms had regained most of the advantage they enjoyed before the MITI VLSI program.[4] Yet that rebound was due in part to the fact that the US and global semiconductor industries of the twenty-first century were very different from those of the twentieth.[5] Some of the giant, dominant firms of the earlier era—especially RCA and AT&T—had disappeared from semiconductor manufacturing. The few large US firms headquartered outside Silicon Valley that remained—especially IBM and Texas Instruments—were now less dominant and much less vertically integrated. Among Silicon Valley's start-ups, as well, a long shakeout had whittled the field down to two leading manufacturing firms—Intel, trailed distantly by Advanced Micro Devices—hovering over a web of auxiliary suppliers and minor players. Most important, the semiconductor industry globalized, with leading firms headquartered in South Korea, Taiwan, the Netherlands, and Scandinavia, and networks of vendors and small manufacturers throughout Asia, Europe, and North America.

In the second half of this chapter, I deal with some of the unforeseen consequences—with respect to American science—of the semiconductor industry's restructuring. Academic microelectronics research centers contributed to that restructuring, but in its wake they had to adjust by becoming more interdisciplinary and more attuned to a variety of other industries. The semiconductor industry's evolution also pushed academic centers into partnerships with a variety of other consortium-like networks spanning non-microelectronic domains of American science. Thus, the increasing consortiumization of American science offered yet another vehicle for the long arm of Moore's Law to effect institutional change even in areas seemingly not related to semiconductors.

Consortium Fever

In the late 1970s, industrial research consortia came to seem like a good and/or inevitable idea in the US semiconductor industry. One reason was simply that other countries were trying such consortia. As one US consortium, SEMATECH, put it in 1990,

> The formation of consortia to address semiconductors and supporting fabrication and material technologies started in 1971 in Japan. Before that, the French and

> other governments had been active in reorganizing their electronics companies into various industrial groups. … Between 1971 and 1980 in Japan alone, five consortia were formed that had semiconductor development activities (including the VLSI Cooperative Society in 1976). In the late 1970s, Korea, the Republic of China, and the United Kingdom experimented with various kinds of joint efforts.[6]

Another reason was that US semiconductor firms were beginning to band together to carry out a broad range of activities, not just research. For instance, the Semiconductor Industry Association and SEMI, a trade group for semiconductor process equipment manufacturers and materials suppliers, formed in 1977 and 1970, respectively—the former, as AnnaLee Saxenian puts it, to "shape legislation or influence elections," the latter to "foster information exchange and collaboration among specialist producers."[7] More generally, the trend since the 1970s across many high-tech industries, but especially semiconductor manufacturing, has been toward increased inter-firm cooperation in a variety of areas, including research.[8]

Semiconductor research consortia were also a product of the gradual disintegration of the Chandlerian multi-division regulated monopoly corporation.[9] Through the 1970s, giant firms such as AT&T, IBM, and RCA had been required, by consent decrees negotiated with the US Department of Justice—to share a significant amount of their research findings and intellectual property in return for regulated quasi-monopoly status. That is, these firms' research arms—especially AT&T's Bell Labs—had acted as informal consortia benefiting American industry as a whole.[10] In the 1980s, though, these firms underwent dramatic changes. As a result, the earlier informal research consortia (upon which Silicon Valley firms had been nearly free riders) were converted into formal consortia with less free riding. The clearest example of this is Bellcore, the research consortium that was carved from Bell Labs in order to serve the regional "Baby Bells" when AT&T was broken up in 1984. More generally, though, as former basic research giants such as Bell Labs and IBM Research moved closer to the Silicon Valley model (in-house research focused on short-term problem solving rather than fundamental discovery), the industry as a whole looked to research consortia to pick up the slack.

From the late 1970s on, for all these reasons and more, microelectronics firms have spent more and more money on extramural, cooperative research. That move was made most visible in the founding of three major consortia: the Semiconductor Research Corporation and the Microelectronics and Computer Technology Corporation (abbreviated MCC), both formed in 1982, and SEMATECH (or Semiconductor Manufacturing Technology), founded in 1987. These were joined by a host of smaller consortia and bilateral or trilateral joint research ventures. By 1990, US firms were spending "31 percent of their semiconductor R&D budgets through technological alliances or consortia … compare[d] with only 11 percent in 1978."[11]

These consortia operated in a variety of ways. Some were focused on narrow topics; others tried to address a broad range of issues. Some were financed entirely by industry; others obtained some of their funding from state and/or federal agencies. A few included the federal government as a member organization. Some had little formal contact with academic researchers; some had contracts with university centers and institutes; some brought together firms, governmental organizations, and universities as co-equal participants.

As I have emphasized in other chapters, institutional experiments such as research consortia rarely work on the first try. Indeed, proponents of consortiumization were aware that their first attempts might fail, but would be worthwhile if they led to further experimentation. As the White House Science Council Panel on Semiconductors put it in 1987,

> The current world competitive situation demands increasing cooperation both horizontally and vertically in the industry as evidenced by the spontaneous emergence of the Sematech proposal. Sematech is not necessarily the ideal instrument, but it is a significant step, a start. Indeed, it is generally agreed even by those advocating Sematech that it will not solve all the industries' problems. However, it will increase communication between elements of the industry, and may encourage new coalitions outside of Sematech, and may even facilitate industry restructuring.[12]

Thus, a few early consortia—most notably the MCC—came to be seen as failures.[13] By the late 1980s, however, the lessons of MCC had been learned well enough that industrial research consortia—much like academic centers programs—became an instinctive response to almost any high-tech development.

The simultaneous turn toward industrial research consortia and industry-oriented academic centers programs was no coincidence. The same economic conditions that gave rise to the one were used to justify the other. As the Economic Policy Council's R&D working group argued in late 1985,

> R&D to develop specific products and processes for the market is indeed the job of industry. Even for such specific product development, however, the market offers inadequate incentive to individual companies. When a company … invests R&D, however, it will share even those producer profits with its competitors as well. … The Administration has already taken important first steps in addressing

> this problem, most notably … the Engineering Research Centers program [which] creates multidisciplinary university centers for research in key general technologies. The program requires that the university obtain industry participation in funding the center and collaboration in the research by industrial scientists or engineers.[14]

In attempting to solve the same problem, therefore, the industrial research consortium and the industry-oriented academic center often came to the same solution. The two organizational forms sometimes overlapped, and each of them often made use of the other in pursuing their mutual goals.

Academic Centers and/as Industrial Consortia

To be sure, academic research centers focused on semiconductors and microelectronics weren't *entirely* novel inventions of the late 1970s. Academic research into semiconductor physics has a long pedigree—in fact, the inventors of the transistor were most worried that they would be scooped not by industrial competitors but by an academic group at Purdue University led by Karl Lark-Horovitz.[15] As center funding became more routine on American campuses it was only natural that some academic semiconductor research would take that form—as happened with some of the ARPA-funded Materials Research Laboratories in the 1960s.

What was new in the 1970s was the frequency of founding of academic microelectronics research centers that were explicitly intended to aid, work with, and receive funding and personnel from the microelectronics industry. One of the earliest such centers—and therefore deserving closer examination for the example it set for later centers—was the Silicon Structures Project at Caltech, led by Carver Mead. We have encountered Mead before as a longtime friend of Intel, and indeed for several years he flew up to the Bay Area from Pasadena every week for a meeting with Gordon Moore.[16] Mead was equally famous for collaborations with other industrial researchers, most notably Lynn Conway of Xerox. Despite his close ties to US firms, he still sometimes found it hard to gain their attention. When he made his optimistic predictions for the miniaturizability of electronic components in the early 1970s, for instance, "the people who were listening the closest were the Japanese," not Mead's American interlocutors.[17]

One of the consequences of the miniaturization that Mead predicted was that integrated circuits became much more complex and therefore the designing of chips became much more labor-intensive. Thus, in the mid 1970s Mead switched from studying scaling and semiconductor physics to developing automated means of designing chips. Here, again, Mead portrayed industry as only belatedly recognizing the relevance of his work, in part because firms were ill-equipped to do long-range research:

> [B]ack in 1970 … there were only a few tens of people-months involved in designing a chip, and everyone said "well, why don't you university folks go and mind your business and play with your toys because we don't think we have the problem." Now [in 1980] it's perfectly clear to industrial people that they have the problem. … One of the functions of a university is to do a fair bit longer look ahead than it's possible to do in a rapidly emerging and very competitive industry.[18]

Here we see the influence of Moore's Law in at least two ways. First, constant miniaturization made product development so rapid and so expensive that most semiconductor firms could only focus on short-term technical issues. But, second, Moore's Law made the characteristics of future chips predictable enough that organizations not under pressure to develop products—such as universities—could take a "longer look ahead."

Pace Mead, however, universities weren't the only organizations that could look far ahead. As we have seen, a few corporate and government labs—among them IBM Yorktown and the Naval Research Lab—were designed to take a longer view. But, as we saw in the cases of Josephson computing and molecular electronics, the further ahead these organizations looked, the more difficult it was for them to translate their findings onto the factory floor. Thus, Mead recognized that he needed some institutional innovation to accompany technological innovations in automated chip design if he were to have any impact on industry:

> [W]e didn't want to have a problem like we had had before where we had gone ahead and done some rather fundamental predictions of what was happening in the future and had found it very very difficult to get the attention of our industrial friends because they were too busy doing what they were doing at the time. So … we decided that we were going to look ahead and try to develop some working relationships with our industrial friends ahead of time instead of after we had done the work. So … we initiated a program called the Silicon Structures Project where we bring in a number of industrial sponsors. Today the list of sponsors is IBM, Xerox, Burroughs, Hewlett-Packard, Digital Equipment Corporation, Intel, and Honeywell.[19]

To put it slightly differently, the university has been a kind of consortium ever since the Middle Ages, but the feudal structure of academia makes universities inefficient consortia with diffuse inputs and outputs. Mead's innovation was to use an academic center to centralize inputs (funding, personnel, research topics) and outputs (findings, personnel, intellectual property) in a way that would give more patrons more direct and accountable returns, just as in an industrial consortium.

And, like an industrial consortium, the Caltech center had in-house "staff" (students) who would work with rotating researchers from the "consortium members" (industrial sponsors):

> Each of these [firms] sends a scientist on a rotating basis to work with us. … It's a built in mechanism of transferring the lore that comes in an academic research environment back into the companies. … It allows the university to do what it's the best at, looking very far ahead, taking risks, looking at things whose outcome is very uncertain. It also uses the industrial organizations for the things they're the best at. And in fact we get a fair bit of help with project management kinds of things and things we'd otherwise have to start stepping outside of our role as a sort of a blue sky research organization.[20]

That is, Mead's center was financed like a consortium, configured technology transfer in much the same way as a consortium, and even brought in corporate project management methods to help it operate less like an academic unit and more like a corporate research lab, albeit one not housed in any particular firm—just like an R&D consortium.

The Silicon Structures Project is relatively well known to scholars of academic entrepreneurship, largely through Elizabeth Popp Berman's study of university-industry research centers.[21] Other early academic microelectronics research centers, especially at MIT and Stanford, have also appeared in studies of university-industry ties, most notably those by Stuart W. Leslie and by Christophe Lécuyer.[22] Yet, for all their strengths, I don't believe these (or other) histories of the entrepreneurial university have put university-industry microelectronics centers properly in the context of the consortium fever that was sweeping the US semiconductor industry in the late 1970s and the 1980s. Nor has the role of the MITI program—acknowledged by Mead in a passage quoted above—been widely recognized as a stimulant to the formation of university-industry centers in the same way that it has for consortia.

Yet the Silicon Structures Project was by no means the only academic center to position itself as a quasi-consortium for semiconductor industry research in the wake of the MITI VLSI program. Both MIT's Microsystems Technology Laboratory and Cornell's National Submicron Facility, for instance, established industrial affiliates programs through which they could interact with industry on a one-to-many basis by presenting their research at affiliates meetings and in newsletters sent to member firms. Such programs gave member firms previews of research and of personnel (both graduate students who could be hired and faculty members with whom firms could collaborate). The numbers for the Submicron Facility's affiliates program are probably typical—by 1986, thirty-seven member firms were each paying Cornell $8,500 per year.[23]

In addition to its consortium-like affiliates program, the National Submicron Facility also partnered with a true industrial R&D consortium by hosting one of the Semiconductor Research Corporation's first three Centers of Excellence in 1983.[24] Many of the smaller projects funded by the SRC, as well, were associated with academic centers such as Stanford's Center for Integrated Systems or the University of Texas' Microelectronics Research Center. As a result, those and other centers had a say in SRC policy through representation on its University Advisory Committee.

One way of thinking about these kinds of symbiotic connections is to understand that, although one purpose of SRC and other R&D consortia is to act as brokers between firms and universities, often these consortia require their own broker and/or outpost on campus in the form of a center. Interfacing with an entire university or even a department is inefficient for a consortium, since those organizational forms rarely make quick or uncontested decisions. Collaborating with a single faculty member guarantees faster response, but not breadth of knowledge—and when the consortium's needs change, an individual professor's expertise may no longer be relevant. An academic center, however, can act relatively quickly and coherently, and can bring the expertise of a broad (but not overly broad) array of faculty members to bear on the consortium's ever-changing needs.

That may explain why, as consortia were forming in the 1980s, they often put their headquarters in states that offered to establish academic microelectronics centers nearby. The Microelectronics Center of North Carolina, for instance, formed as a five-school state-funded academic consortium in 1981, and was an important factor in North Carolina's success in attracting the Semiconductor Research Corporation in 1982.[25] Similarly, when the state of Texas wanted to woo the MCC to Austin in 1983–84, part of Governor Mark White's pitch was that the state would put money into a new Microelectronics Research Center at the University of Texas in Austin.[26] Then when Texas wanted SEMATECH to put its headquarters in Austin, in 1986–87, it built the MRC a new state-of-the-art facility.[27]

Texas wasn't the only state to try using an academic center to get SEMATECH. As Stuart Leslie has shown, New York's Albany region offered the consortium a Center for Integrated Electronics at Rensselaer Polytechnic Institute, but "in the prevailing political climate, Texas simply had more clout than New York."[28] Perhaps the prevailing climate has changed, though, since the Albany region poached SEMATECH's headquarters away from Austin in 2010, in large part by promising that the State University of New York system would build a Computer Chip Commercialization Center, a Chemical Mechanical Planarization Center, and other centers as the consortium's local academic partners.[29]

Some academic microelectronics centers partnered with consortia of a different kind: local high-tech incubators. In the 1980s incubators became a popular tool for universities to foster high-tech start-ups, especially those founded by faculty members or by former students. Obviously, an incubator is different from a consortium such as SEMATECH or SRC. Still, incubators attempt to consortiumize many of the things most relevant to high-tech entrepreneurs: real estate, pools of investors, administrative costs, personnel, expertise, and so on. And like the larger R&D consortia, incubators sometimes find it useful to deal with academic centers rather than with university departments. Similarly, centers sometimes find that a relationship with an incubator makes it easier to partner with firms.

A good example of a microelectronics-oriented center-incubator relationship comes from the Center for Nanotechnology at the University of Washington, formed in the late 1990s from a loose coalition of surface scientists and bioengineers spread across several departments. Most of the CNT's initial funding from both the university and the National Institutes of Health went to buying new nanofabrication and nano-characterization tools and to the expansion of an existing microfabrication user facility in a university-affiliated tech incubator, the Washington Technology Center. The WTC Microfabrication Laboratory was then renamed the NanoTech User Facility (NUF) and reoriented to place greater emphasis on biomedical microfabrication. As the proposal to create the NanoTech User Facility put it,

> other Centers have experienced that well maintained user facilities act as focal points to build long-lasting relations between industry and research institutions. They create an environment in which scientists from industry can meet and collaborate with students, post-doctoral fellows, and faculty while pursuing mutual research interests.[30]

Indeed, sometimes centers act as more than just "focal points." Because clean rooms and semiconductor process equipment are so expensive, cash-poor start-ups may find renting time on academic equipment much more desirable than buying their own tools and building their own fab. In at least one instance I know of (at the University of Texas), a local start-up had semi-permanent office space within an academic microfabrication user facility. That may have been a slightly unusual arrangement, but other such facilities I have visited have been very happy to have local start-ups use their equipment almost daily.

Even an academic microelectronics center that wasn't associated with a start-up incubator and didn't have a consortium-like affiliates program could still take advantage of other consortiumized activities of local industry. A good example is the University of Minnesota's Microelectronics and Information Science (MEIS) Center, which was designed to allow users access to "the processing facilities of nearby corporate contributors, including Control Data, Honeywell, Sperry, and 3M."[31] That is, local firms consortiumized their equipment base through the academic center.

Those firms' cooperation to support the MEIS Center was amplified, in turn, by their consortiumized political lobbying. As one of the founders of the center (Ray Warner) recounted, when he wrote a memo to the president of the university in 1983 complaining about the Electrical Engineering Department's dire lack of resources, he "also sent the memo to the Minnesota High-Tech Council (MHTC), a fairly active committee of local captains of industry, and the Governor." "The President," he continued,

> seems not to comprehend … that a quarter of the State's manufacturing economy is electronics-related, and that segment depends heavily on the ~230 people we turn out each year—they recruit relatively few from outside the state. Well, this week the President was called on the carpet by the Governor and the MHTC. They reportedly were fairly blunt: We think it is urgent for you to "stabilize and then rebuild" the EE Department. … Moral [*sic*] here is higher than it has been for years, with the thought that we finally found a club big enough to get his attention.[32]

We saw something similar in chapter 4 in Cornell's use of its industrial users, collaborators, and advisers to lobby the National Science Foundation to continue funding the National Submicron Facility. Such lobbying is, again, similar to the practices of industrial consortia, which have had to rely on the combined political clout of their participating firms in order to secure federal and state support and favorable legislative changes.

Almost all of the variations on the center-consortium relationship that I have described above were taken a step further than anywhere else at Stanford's Center for Integrated Systems. Recall that the CIS was founded in 1980 with financial support from a small but powerful group of industrial affiliates and with large grants for equipment acquisition and research from DARPA and the NSF. The initial pitch to those stakeholders had two major components: "the essential vertical integration of disciplines in integrated systems, going from solid-state physics to computer and communication systems" and "the desirability of a much higher level of university-industry connection."[33]

For its part, the National Science Foundation concurred that its support for the CIS was predicated on disciplinary "integration" and "university-industry connection." As Erich Bloch put it,

> the ideas that led you to create the center are the same ideas that are becoming the basis for the nation's new policies for the funding and organization of scientific and engineering research. … The establishment of this center recognizes and embodies several important principles, specifically:
>
> - The emphasis on industry-university partnerships
> - The deliberate integration of research efforts across traditional disciplines
> - The focus on areas of research that can underpin advances in technology, and thereby help our manufacturing industries to compete better in the international economy
>
> Being founded on these principles, the work of the center becomes a good illustration of the linkage between scientific research and industrial competitiveness that national policymakers are attempting to foster in universities.[34]

It is worth noting that Bloch was the founding CEO of the Semiconductor Research Corporation immediately before moving to the NSF to carry out the administration's agenda of supporting "industrial competitiveness" through initiatives such as Engineering Research Centers program.[35]

For its industrial sponsors, meanwhile, the CIS offered a way to share the burdens of research—an aim most of the sponsors were simultaneously trying to achieve through participation in industrial consortia. The price of affiliating with the CIS was at least an order of magnitude more than the price of membership in most other academic centers' industrial affiliates programs, though still about an order of magnitude lower than the price of membership in a research consortium, and thus it was much more of a hybrid center-consortium than its peers. Specifically, for the first eighteen CIS affiliates, the cost in 1982 dollars was an up-front payment of $750,000 plus annual dues that started at $100,000 (equivalent to roughly $1.8 million and $250,000 in 2016 dollars).[36]

Some executives at CIS sponsor companies were fairly explicit that the center—like the new industrial consortia—would make up for Silicon Valley firms' declining free ridership on the basic research output of Chandlerian firms. Les Hogan of Fairchild—a man both legendary and infamous in the history of Silicon Valley for precipitating the departure of Gordon Moore and Robert Noyce from Fairchild to found Intel—was quoted as follows in a 1982 issue of the *CIS Newsletter*:

> "In the 1960's and 1970's, the semiconductor industry was living off the enormous amount of basic research that was performed in the 40's and 50's," says Dr. Hogan. He feels that this legacy has been exhausted, however, without the benefit of basic research to take its place. … Dr. Hogan has long advocated cooperative efforts in basic research by industry and academia. … He regards Fairchild's funding contribution to CIS as an act of "enlightened self-interest."[37]

And if American firms and universities didn't cooperate out of "enlightened self-interest," Hogan knew who would:

> Dr. Hogan points out that Japan, whom he feels is America's main competition in high technology, has employed somewhat the same approach to basic research on problems crucial to Japan's economic future. … "To win against foreign competition," says Dr. Hogan, "we need programs like CIS, sponsored by industry, at a half dozen more of our best universities, coupled with renewed vigor for basic research by industry itself."[38]

Not all microelectronics executives were as enthusiastic about the CIS as Hogan, of course—not even Robert Noyce, even though, as was noted in a 1981 article in the *San Jose Mercury*, Intel "became the third Silicon Valley company to sign up [as a CIS sponsor], after banner-waving Hewlett-Packard Co. led the way." The article continued as follows: "'We have to support this sort of thing even if it is not in the exact form we would like,' Noyce said. Apparently not all other corporations agreed. Noyce said about half the companies approached gave a thumbs-down response."[39] These skeptics' concerns about the CIS were the same worries often expressed about industrial consortia. Noyce, for example, complained that "the benefit is spread much more broadly than the cost" and that "the non-participants in CIS will benefit just as much as the participants." "Unless a participating company looks at the broader benefits," he continued, "the winning strategy is not to participate."[40]

The Corporate Microelectronics Diaspora

Industrial semiconductor research consortia and industry-oriented academic microelectronics research centers resembled one another in their aims, in their operations, in their patrons, and in the rhetoric of both their proponents and their detractors. And, whatever their detractors might say, both organizational forms proliferated in the 1980s because firms were less and less willing to do long-range research in house.[41] The result was that a whole generation of corporate physical and engineering scientists could see fewer opportunities to do curiosity-driven fundamental research within their firms, but greater opportunities for extramural industry-oriented basic research in consortia and academia. Moving out of industry and into quasi-industry therefore became a very attractive option.

Some migration of industrial researchers into universities was, of course, a long-standing tradition. Recall that Fred Terman's strategy for building up Stanford's reputation in electrical engineering in the 1950s and the 1960s was to hire professors away from places such as Bell Labs. The classical pattern, though, had been for those faculty members to train students who would go into industrial basic research and who would then return to academia only to start the cycle over again. In contrast, what happened in the 1980s and the 1990s was a one-time release of basic researchers from industry, with no restocking of the pool. That exodus was most evident at the companies that had been most committed to the old model of doing basic research in house. As the Economic Policy Council noted in 1985,

> long-term research that bears on whole technologies, as opposed to specific products and processes, is for the most part too expensive and risky, and its benefits too hard to capture, for any single company. (The most notable exception has been Bell Laboratories, which became a unique national resource. But divestiture and deregulation, whatever their other benefits, have seriously disrupted the special conditions that allowed Bell Labs to develop its unique character.)[42]

Even before the official breakup of AT&T in 1984, Bell Labs was becoming a less attractive employer for some researchers. The result was a remarkable—if transitory—jolt to the quality of American academic microelectronics research as top-shelf applied physicists and electrical engineers left Bell Labs to seek university positions.

Academic microfabrication centers, in particular, benefited from an influx of Bell Labs personnel. AT&T, after all, was a leader in microfabrication, having invented molecular-beam epitaxy and having been first to use an electron-beam lithography system (the EBES) in commercial microelectronics manufacturing. In the first five meetings of the Gordon Research Conference on the Chemistry and Physics of Microstructure Fabrication held between 1976 and 1984, Bell Labs personnel formed the largest single contingent, numbering 53—more than the combined contingent from MIT and Lincoln Lab (49) and more than the number of attendees from multiple IBM locations (47).

From 1984 on, though, Bell Labs' participation in the Gordon Conference declined, and leadership of the conference increasingly fell to former (or soon-to-be-former) Bell Labs researchers in academia. The 1984 meeting was organized by Stanford's Fabian Pease (who had been at Bell Labs until 1978), the 1986 meeting by the University of California at Santa Barbara's Evelyn Hu (who had been at Bell Labs until 1984), the 1990 meeting by Cornell's Harold Craighead (BTL and then Bellcore until 1989), the 1994 meeting by Don Tennant (who would move from Bell Labs/Lucent to Cornell in 2006), and the 1996 meeting by MIT/Lincoln Lab's Margaret Stern (who had been at Bell Labs until 1982 or 1983). As we'll see, these individuals and the schools with which they were associated would steer academic microfabrication in a dramatically different intellectual and organizational direction in the 1990s.

By the late 1980s, other firms were beginning to join Bell Labs in shedding research personnel. For instance, the number of attendees from IBM at the Gordon microfabrication conference fell from 47 over the first five meetings (1976–1984) to only 30 over the next seven (1986–1998). In general, academic and industrial attendance at the conferences flipped as the Cold War came to a close. As figure 5.1 shows, industrial attendees

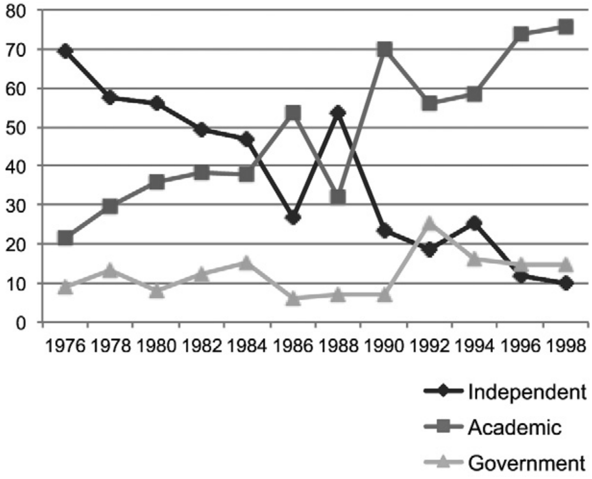


Figure 5.1
Percentages of attendees from academic, industry, and government organizations at the Gordon Research Conference on the Chemistry and Physics of Micro/Nanostructure Fabrication. I have categorized attendees using criteria applied by the GRC home office for the 1976–1990 meetings. I applied my own criteria (based on attendees' addresses) for 1992 and 1994. Attendees self-reported organizational type for the 1996 and 1998 meetings. The 1992 figures are approximations based on speaker and poster lists; no full attendee list was available. Source: Gordon Research Conference collection, Chemical Heritage Foundation archives.

outnumbered their academic counterparts (by decreasing amounts) in every meeting from 1976 to 1988 except for 1986; conversely, academics outnumbered corporate researchers (by increasing amounts) in every meeting from 1986 to 1998 exclusive of 1988.

The unprecedented movement of microelectronics researchers from firms to universities happened just when the semiconductor industry's and federal policy makers' panicked justifications for research consortia and consortium-esque academic centers began to ring hollow. With the end of the Cold War, there was no longer a Soviet threat which the United States needed to stay ahead of, nor was there much foreseeable chance that any enemy could (much less would) disrupt shipments of military-relevant circuits from East Asia to the United States. At the same time, the economic rationale for federal support began to look less compelling as Japan's economy stalled and American semiconductor firms regained market share. After a few missteps, consortia in the semiconductor industry began to work as hoped, leading to new consortia targeted to almost every aspect of semiconductor manufacturing: R&D, political lobbying, market planning, development of process equipment, transitions to larger wafer sizes, and so on. Nationalist rhetoric surrounding the "loss" of American manufacturing dominance began to decline, even as semiconductor and electronics manufacturing really did globalize to a tremendous extent. In fact, some consortia that had been founded on nationalist grounds began to open up to international participation. For instance, the US National Technology Roadmap for Semiconductors, which had begun issuing annual industry-wide targets in the early 1990s, became the *International* Technology Roadmap for Semiconductors in 1998.[43]

We can see the same civilianization and globalization in the post–Cold War evolution of academic microelectronics research centers such as the Center for Integrated Systems. In truth, despite the nationalist rhetoric that often accompanied early statements about or in support of the Center for Integrated Systems in the 1980s, some of that facility's leadership had long contested stark US-versus-Japan rhetoric. John Linvill put it this way in 1981:

> Suppose, for some reason, whether it's their ability in hardware … or whether it's a combination of the nature of the people and their heritage; just suppose they [Japanese firms] turn out to make 16K RAMs significantly better than any American manufacturer; then I say we ought to buy it from them and send them the things we do better than they do. In other words, we need to optimize the system which includes the United States and Japan, decide who does what best by competitive means.[44]

Still, despite Linvill's expressed openness, Asian firms were slow to join and/or slow to be recruited to the CIS affiliates program. Even though two of the CIS' original corporate sponsors (Signetics and Fairchild) were owned by European firms, and two more European firms joined in the early 1990s (Siemens in 1993, Ericsson in 1994), it wasn't until 1998 that an Asian firm (Hitachi) became a sponsor. A second Asian sponsor, the Taiwan Semiconductor Manufacturing Company (TSMC), didn't join until 2001, more than twenty years after the CIS was founded.

The inclusion of the TSMC among CIS' sponsors was a signal of the semiconductor industry's post-vertical integration structure. As the *CIS Newsletter* noted,

> TSMC's charter prevents it from designing or making its own brand-name IC products. Therefore, TSMC is totally committed to producing high volumes of state-of-the-art semiconductor technology for its customers, who then apply the technology to their own product designs. … With TSMC as the engine of change, what was once only a concept—a pure foundry—is today a multi-billion dollar industry. As the semiconductor industry faces spiraling IC fab construction costs, dedicated foundry companies like TSMC stand to become the primary sources to meet world wide demand.[45]

That is, because Moore's Law necessitated the use of ever-more-expensive equipment to make ever-smaller transistors, the cost of building a state-of-the-art semiconductor fab rose so much that very few firms could both design chips *and* build and run a fab dedicated to producing those chips. In the same way that research consortia allowed microelectronics firms to transfer some in-house research outside their walls and fund it cooperatively, foundries such as the TSMC allowed firms to transfer some (or all) in-house manufacturing outside their walls and fund it cooperatively. If the "research" side of the National Research and Resource Facility for Submicron Structures or the CIS was co-emergent with research consortia such as the SRC, then the user facility/"resource" side of the NRRFSS—or the Integrated Circuits Lab portion of the CIS—was co-emergent with foundries such as the TSMC.

We saw in chapter 4, though, that the NRRFSS had trouble keeping its research and resource missions under one roof. Similarly, as foundries such as TSMC drove firms to spin off portions of the design-to-manufacturing cycle, the Center for *Integrated* System's original model—integration from solid-state to systems—made less and less sense. As James Plummer, formerly a graduate student who worked on the Optacon and later served as director of the Integrated Circuits Laboratory (the microfabrication unit of the CIS) put it in a 2010 interview, CIS sponsors originally "were all vertically integrated companies. So, different pieces of different companies interfaced with different parts of the CIS structure here. … It's different today because many fewer companies are vertically integrated. Fabs are no longer commonplace in most companies, because of the existence of foundries. So the CIS model has had to evolve over time to recognize that many of the companies that belong to it are not vertically integrated and just connect in at specific points in this hierarchy."[46]

Thus, technological, business, *and* geopolitical shifts in the late 1980s and the early 1990s all conspired to force academic organizations like the CIS, MIT's Submicron Structures Laboratory, and the Cornell Nanofabrication Facility—and their federal and corporate sponsors—to rethink how these experiments in university-industry linkages should operate.

From Writing Circuits to Reading Genes

In 1988, the CIS Industrial Sponsors Advisory Committee reflected on what political, technological, and economic changes would mean for academic microelectronics research:

> How has the competitive environment changed in the last 5 years? … Long range research in American Industry has decreased and companies have emphasized short range results. Globalization of business has proceeded, while globalization of research has been left behind. Research consortia have become a new mode of cooperation within countries.[47]

As a result, the committee suggested that the facility "abandon the concept of 'integrated systems' which is difficult to support and detracts from the important successes achieved by CIS."[48]

Two years later, after the dismantling of the Berlin Wall, John Linvill wrote Gordon Moore to report on a new round of re-thinking:

> We want to concentrate on "paradigm shifts." … A shift from pure university teams to FMA teams [faculty member-corporate mentor-graduate advisee—a more industry-directed approach] has clearly occurred and we have a different operation as a result. In IC fabrication there is a shift of attention from large facilities to micro-factories [i.e., foundries]. The emphasis in mission identification of our government sponsors has moved from national security to a dual role of preserving military security and furthering our national competitiveness at the same time. … We should be thinking about the relationship that we should be building with international companies, Asian and European, in the years ahead after security is more assured.[49]

In other words, although the CIS' patrons were still focused on its "dual role of preserving military security and furthering our national competitiveness," they were now much less anxious about existential threats to domestic semiconductor manufacturing from Japan or existential threats to the US from the USSR. What preoccupied them now was getting corporate objectives more directly into the CIS' research agenda, which they accomplished in part by making sponsor firms' representatives co-advisees of CIS-affiliated graduate students' theses.

Left unsaid in Linvill's letter is that the end of the Cold War was also beginning to catalyze a "paradigm shift" in the hierarchy of scientific disciplines and federal grant agencies. In the federal budget for fiscal year 1992, which was already under discussion when Linvill composed his letter, funding for the National Institutes of Health rose sharply while federal funding for the physical sciences peaked and began a long drought.[50] At its peak, in fiscal year 2002, the NIH's budget was almost three times what it had been (in constant dollars) when Linvill wrote Moore, even though the amount budgeted for physical science had fallen in the same period. Funding for the engineering sciences, too, was essentially flat for the entire decade of the 1990s. Federal funding for defense R&D, meanwhile, peaked in fiscal year 1987, declined until the mid 1990s, then remained flat until after the attacks of September 11, 2001, whereas federal spending on civilian R&D began rising (intermittently) in the early 1980s until, on the eve of 9/11, it nearly equaled federal spending on defense R&D .

In that environment, it made sense for academic microelectronics and microfabrication specialists to look to new funders and hence to new topics, especially ones connected to the life sciences. And in making that pivot, academic centers benefited from the fact that, although they operated much like industrial research consortia, they possessed degrees of freedom that purely industrial consortia did not. Thus, academic microelectronics centers could look for partners in industries other than semiconductors, in university departments other than the physical and engineering sciences, and in other academic centers at their own university or elsewhere—opportunities largely denied to SRC or SEMATECH. The leading academic centers tried all those approaches—perhaps none more so than Cornell's National Nanofabrication Facility and Stanford's Center for Integrated Systems. Indeed, those two centers were so adept at multi-industry partnerships and interdisciplinary, inter-center, and inter-university collaboration that they eventually joined forces through a complex arrangement that might be thought of as a consortium of consortia or a network of networks.

The widening of these centers' scope began rather modestly, however—and at Cornell somewhat humorously. Publicly at least, administrators of the Submicron Facility claimed from the beginning that they were looking for interdisciplinary projects that spilled beyond traditional silicon microelectronics. Even in 1980, facility newsletters praised "the quality, dedication, and interdisciplinary attitude of the faculty members associated with the NRRFSS," the latter exemplified by an oft-repeated list of the disciplines represented in the facility: "user projects at NRRFSS have embraced not only electrical engineering, physics, and materials science, but also biology, medicine, and agriculture."[51] Actual projects undertaken by faculty members during the ten-year existence of the NRRFSS/National Submicron Facility, however, were heavily weighted toward questions from microelectronics, condensed matter physics, and materials science.

There were, to be sure, a few life-sciences-oriented projects underway in those early days. For instance, Myunghwan Kim, a Cornell professor of electrical engineering, used the facility to fabricate chips for probing the nervous system of a cockroach—work that was featured in *Popular Science* and in *Newsweek*.[52] Yet as late as 1988, of the 48 external projects then underway at the National Nanofabrication Facility (NNF), only four were led by biomedical or life-science organizations: the University of Pennsylvania's Department of Orthopedic Surgery Research, two projects from the University of Rochester Medical Center's Department of Biophysics, and one from the Marine Biological Laboratory in Woods Hole; the latter seems to have been a microfabricated test pattern for calibrating video cameras on submersibles.[53]

Similarly, of the 58 Cornell projects based in the NNF at that point, only *one* was housed in a life-science department—a department of plant pathology. Certainly, that one project was quite a striking example of what could be done in microfabrication–life science collaborations. It involved fabricating a set of precisely varying topographic features on a polystyrene substrate and then introducing a rust fungus onto the substrate to see which features stimulated the fungus to attempt to penetrate its "host's" epidermis—with a view both to fundamental discovery and for application in breeding fungi-resistant plant varieties.[54]

In the end, it was up to successive directors—first Edward Wolf and then Harold Craighead—to put the life sciences in the facility on a permanent, prominent basis. But at first glance neither director was a likely candidate for that role. Both had been educated in the physical sciences (Wolf in chemistry, Craighead in applied physics) before moving into industrial research (Wolf at the Rockwell International Science Center and then at

Hughes Research Labs, Craighead at Bell Labs and then Bellcore) and making their names in microfabrication. And, as his pedigree might have predicted, Wolf's initial foray into the life sciences was largely accidental. It came about in 1982 when a Cornell horticulture professor, John Sanford, gave a talk to his colleagues about his unpromising attempts to drill holes in pollen grains with a microlaser "with the purpose of letting DNA diffuse through the opening in the [pollen grain] wall."[55] One of those colleagues happened to have heard of the Submicron Facility and suggested that it might have more precise beams that could do the same job better. Not knowing anyone at the facility, or anything about microfabrication, Sanford approached Wolf, the facility's most visible representative. Interested but not sure that the facility could actually help, Wolf entered into several months of discussions with Sanford. The discussions culminated in the idea of accelerating the DNA through the intact cell walls of the target plant, rather than drilling holes in the cell walls and letting the DNA drift into the plant. Once Sanford estimated the velocity needed for transplantation, Wolf "immediately realized that such speeds … could be achieved using a variety of relatively unsophisticated technologies, and certainly did not require ion beams and electrostatic accelerators."[56] Their first successful gene "accelerator" was a toy air pistol loaded with tungsten powder. To test whether powder coated with DNA could harmlessly enter plant cells (and then allow the DNA coating to enter the plant's genome), Sanford, Wolf, and Nelson Allen (a Submicron Facility machinist)

> during the Christmas break of 1983 … went into the Submicron Facility (which required all of us to don white gowns, booties, and hats), and surrounded by various multi-million dollar ion beam accelerators, we proceeded to blast whole onions. … The air reeked of onion—onion juice and bits of onion were all over our sophisticated high-tech frocks. … We went home that night amused by the contrast between the sophistication of the Submicron Facility and our very primitive and seemingly foolish antics with toy guns and raw onions.[57]

Amused, but also on track toward commercial success. Sanford, Wolf, and Allen soon applied for a patent, with Cornell's office of technology licensing (then known as the Cornell Research Foundation) as the assignee. Then, in 1986, Sanford, Wolf, and their wives formed a company—with Cornell taking an equity stake and Wolf's son-in-law as "manager and only real employee"—to license the gene-gun patent and sell copies to researchers. In 1990, Du Pont bought the company, generating what the STS scholar Nicole Nelson describes as "the largest royalty payment to the Cornell Research

Foundation up to that date and … one of the most 'readily recognized financial successes' in the history of Cornell technology transfer."[58]

The story of the gene gun exemplifies the narrative of American science told in this book. Ed Wolf was an early pioneer of what has become a common trajectory: a successful career in corporate research, then a move into an administrative position in academic engineering science, exploration of interdisciplinary collaborations (especially with the life sciences), and finally a move toward entrepreneurship. We have seen an earlier instance of a similar trajectory in the careers of John Linvill and James Meindl, but even in the mid 1980s Wolf's path from corporate microelectronics researcher to academic life-science collaborator and start-up founder was quite novel. As industrial microelectronics researchers began flocking to academia in the late 1980s, though, Wolf came to seem more like a pioneer than an anomaly.

Under Wolf's successor, Harold Craighead, the (renamed) National Nanofabrication Facility became a much more prominent host of ties between microfabrication and the life sciences. That move is particularly evident in Craighead's 1994 proposal that the facility "continue to seek out interactions with life scientists and projects where sensors and micromechanical devices under development for other areas may find new application, particularly in the fields of biology, medicine, and plant and soil sciences" and in his comment that "NNF considers bioengineering and life sciences projects an exciting area where microfabrication increasingly has significant impact."[59]

Craighead's reasoning was based in part on a conference on "Nanofabrication and Biosystems: Frontiers and Challenges" that he organized earlier in 1994 with two other Cornell faculty members: Lynn Jelinski (a biophysicist) and Harvey Hoch (the plant physiologist who conducted the abovementioned studies of rust fungus at the National Submicron Facility). In the course of the 1990s, Craighead's research turned increasingly toward microfabrication for biophysical applications. In 2000 he became the founding director of Cornell's Nanobiotechnology Center.

The vision that Craighead and his colleagues put forward invoked two mutually reinforcing tracks: microfabrication expertise to aid research in the life sciences and life-science expertise applied to developing new microfabrication techniques.

> In addition to its importance in the microelectronics industry, microfabrication can be used to produce small devices with electrical leads that have the dimension of cells. This makes it possible to use microelectronic devices to begin to solve, and interface with, biological problems. Conversely, nanofabrication

> is approaching the size limits of current technology. It may be possible to use some form of biological self-assembly to produce features that are smaller or have more difficult aspect ratios than is currently possible. Biological molecules self-assemble into highly organized supramolecular structures, and it may be possible to harness some self-assembling features of biomolecules to push nanofabrication to even smaller limits. These two complementary areas of research appear to be coming together.[60]

Note how Moore's Law underwrites both halves of that logic. By scaling down circuit components to the size of cells, it makes microelectronics relevant to life scientists; but by demanding further scaling, it provides an opening for life scientists to contribute to new methods of miniaturization. As I will discuss in the next chapter, in the 1990s the convergence of inorganic microfabrication with organic chemistry and molecular biology led to new research in molecular electronics and provided the rationale for a number of academic centers that, like Craighead's, adopted "nanotechnology" as their brand.

The Cornell facility's embrace of the life sciences was mirrored by Stanford's Integrated Circuits Laboratory, though perhaps the ICL's directors encouraged interdisciplinary collaboration from afar rather than diving into it themselves as Wolf and Craighead did. We saw in chapter 1, of course, that James Meindl had in fact encouraged *and* participated in a wide range of collaborations with the life sciences after taking over as director of the ICL in the late 1960s. By the mid 1980s, though, he and the ICL in general had retreated somewhat from that kind of interdisciplinarity.

As evidence of that retreat, recall that in the mid 1980s the ICL's major patrons were the semiconductor firms that sponsored the CIS, plus DARPA and the NSF. In contrast, Meindl and Linvill's work in biomedical and disability technology in the late 1960s and the early 1970s had been sponsored by the National Institutes of Health, the Office of Education, and by NASA, as well as by the Navy and the NSF. Other Stanford electrical engineers who affiliated with the CIS in the 1980s had *in the 1970s* been supported by the NSF's Research Applied to National Needs program and the Energy Research and Development Agency. Some had even taken part in projects funded by the National Endowment for the Arts. That is, in the early 1970s "integration" around Stanford microelectronics ran *horizontally* across many specialties (and hence many agencies), whereas in the 1980s CIS emphasized integration that ran *vertically* from "from solid-state physics to computer and communication systems."[61] As a result, specialties and agencies only tangentially related to the manufacturing of semiconductors or of computers were represented only tangentially in the CIS. By 1985, of

173 CIS-affiliated faculty projects, only eight (overseen by three different principal investigators) dealt explicitly with medical applications.[62] Three projects (all of them under the same PI) dealt with environmental monitoring and were funded by NASA, and a further three (under two PIs) dealt with energy-related topics and were funded by the Department of Energy and by a consortium of electric utilities. Those numbers aren't insignificant, but they do contrast with the days when the Optacon and various biomedical technologies had been the mainstays of the ICL.

Certainly, the collaborations with music, philosophy, psychology, science studies, and other humanities and social science disciplines that had characterized Stanford's School of Engineering in the early 1970s were absent from the CIS in the 1980s, or were present only insofar as they had something to say about semiconductor manufacturing. Only one social scientist, Daniel Okimoto, was a member of CIS during this period, on the basis of a project on the political economy of the semiconductor industry in the United States and in Japan. Similarly, the CIS had one affiliate from the Graduate School of Business conducting work with Meindl (funded by the Semiconductor Research Corporation) on "factory modeling and management" of semiconductor fabs. Only one (unfunded) research project in the Graduate School of Education came somewhat close to the earnest, questing interdisciplinarity of fifteen years earlier: "interests include use of technology in the elementary and secondary schools, ways of facilitating access to emerging knowledge by non-specialists in the development of educational software for microcomputers, and the CIS as a model for a new form of educational exchange."[63]

Pointing out the differences between the more restrictive and "vertical" notion of interdisciplinarity that characterized the CIS in the 1980s and its founders' work around 1970 should not, of course, be taken as a *criticism* of Stanford microelectronics in the Reagan era. Rather, I am simply mapping out the administrative and epistemic landscape of the CIS—and its differences from the landscape of electrical engineering at Stanford during the Optacon days—in order to draw a connection to the contemporaneously changing industrial and technological landscape of semiconductor manufacturing. This book is not meant as an unreflective celebration of any kind of interdisciplinarity, or of the socially relevant research agenda of the Vietnam era. Instead, its aim is to show how the evolving organization and capabilities of a powerful industry echoed throughout the entire apparatus of the American scientific enterprise.

Although the unease about military funding that had gripped Stanford in the late 1960s had abated by the mid 1980s, it hadn't disappeared entirely.

Thus, members of the ICL faculty still perceived a need to diversify beyond military funding. But whereas in the Vietnam era diversification had meant forging links with civilian funding agencies and philanthropies and projects to benefit sympathetic populations, in the Reagan era diversification meant partnering with industry. As James Gibbons (one of the leaders of the CIS at its founding and dean of engineering from 1984 to 1996) put it,

> The vast majority of the support for research in the CIS comes from the government, the Defense Department in particular, and is likely to continue that way for some time. "However, there is an increasing discussion on campus about the propriety of accepting funding from the Department of Defense for research. … I think we can agree that it would be good if we could distribute our base of research support, so as to be less dependent on *any* agency of the federal government. So let's consider how we might reduce our dependence on them."[64]

As John Linvill argued somewhat more strongly, the CIS' industrial sponsorship

> "broadens our academic freedom" by diversifying the funding of research. Private industry will provide a different perspective on research than the federal government.[65]

Thus, the entrée for the life sciences to regain traction in the ICL had to come from industry, with only belated encouragement from government. Of course, the CIS/ICL was located at the edge of Silicon Valley, and therefore the industry that most strongly influenced it was the semiconductor industry. Yet it was also located next to one of the oldest and largest clusters of biotechnology firms in the world. Thus, the needs, resources, and intellectual challenges offered by the biotechnology industry slowly began to attract attention within the CIS/ICL. Perhaps the best example of that trend—and of the related migration from industrial basic research to academic industry-oriented research—is Fabian Pease's role in the development of DNA microarray or "gene chip" (trademarked as GeneChip) technology.

As Tim Lenoir and Eric Giannella have shown, the DNA microarray arose through a *mutual* spillover of industrial research into academia and vice versa.[66] The leading manufacturer of DNA microarrays, Affymetrix, was a spin-off of a second-generation biotech firm, Affymax, looking to develop high throughput methods for varying and screening molecules for drug discovery. The management of Affymax was also becoming aware in the late 1980s that the nascent Human Genome Project could potentially be a large market for rapid DNA-sequencing techniques. Doogab Yi describes what happened next: Two Affymax researchers, Leighton Read and Michael Pirrung, "drew an analogy with the production of silicon chips using photolithography. … Following an innovative technology used in the semiconductor industry, VLSI (very large-scale integration), they decided to develop VLSIPS (very large-scale immobilized polymer synthesis)" as a means for creating a large, random library of molecules for rapid screening.[67]

Since Affymax didn't have any resident expertise in VLSI but did have very close ties to Stanford, its researchers naturally used those ties to locate a Stanford VLSI expert. They were referred to Fabian Pease. Recall that in chapter 4 we met Pease as he was leaving Bell Labs to compete with Edward Wolf to head the NRRFSS, and that he was later to joust with Michael Isaacson of Cornell to win the famous Richard Feynman microfabrication wager. Pease's graduate student Tom Newman won that wager in 1985 by writing a page of *A Tale of Two Cities* at 1/25000 the size of standard print with an electron-beam lithography machine. By the late 1980s, though, it was becoming increasingly clear that electron-beam lithography would not replace optical lithography as the mainstay of semiconductor manufacturing. Though the theoretical resolution limit of electron beams was, indeed, finer than optical photons, electron-beam lithography would always be slower than optical lithography, whereas optical lithography was proving surprisingly scalable.[68] Thus, the intellectual challenges offered by Affymax must have seemed refreshingly open-ended and more likely to see application than Pease's previous work on electron beams.

Lenoir and Giannella describe what happened next:

> [Stephen] Fodor [of Affymax] and [Lubert] Stryer [of Stanford, but on Affymax's scientific board] persuaded Pease to join Affymax as a consultant on their project, and he and Fodor spent a lot of time discussing technical aspects of lithography needed to build the microarray. Pease took Fodor around to various warehouses in Silicon Valley to acquire old lithography instruments needed for building the prototype peptide array. By May, 1990 with periodic input from Pease, Fodor had a working semi-automated lithography instrument that would do binary combinatorial peptide synthesis. Pease maintained his connection to Fodor after the launch of Affymetrix in 1992. In 1993–94, for instance, he took a sabbatical from Stanford to work on the DNA microarray. Pease has been co-inventor along with Fodor and Stryer on several key Affymetrix patents, and he has continued to maintain a consulting relationship with Affymetrix.[69]

In other words, though Cornell's Submicron Facility and Stanford's CIS began life as quasi-consortia oriented toward the microelectronics industry, the revenues they generated for their schools from intellectual property were grounded in partnerships with the *biotech* industry. That degree of flexibility—even, in the best sense of the word, opportunism—would be hard to imagine in a more purely industrial consortium such as SRC or SEMATECH.

Such opportunism was a natural mode of operation, though, for the generation of microelectronics researchers—such as Pease, Wolf, and Craighead—who left corporate labs for university positions from the late 1970s on. They brought with them tools, contacts, and a commitment to the zig-zag model of innovation, but little allegiance to traditional academic disciplines. By the 1990s, enough corporate microelectronics researchers had moved to universities and formed partnerships with life scientists to create a bandwagon of sorts. As Stephen Edgington, editor of *Bio/Technology*, wrote in 1994 while describing a "gel-on-a-chip" technology fabricated by a Princeton professor at Cornell's National Nanofabrication Facility,

> Nanotechnology—the ability to create devices that are submicron in size—is poised to give biotechnology some of the tools it has been dreaming about. No longer the exclusive domain of the computer industry, these new techniques, combined with government-sponsored programs, now make the technology within reach of any lab. And when a technology comes along that allows every lab's dreams to be implemented, it is likely to open up lines of inquiry that no one has ever considered before.[70]

I have written elsewhere that the life sciences returned the favor by providing a life-line to a stagnating academic microfabrication community.[71] As a Gordon Research Conference monitor reported from the 1988 Microstructure Fabrication meeting, "there might be an ingrowing clique forming which might be deleterious to branching out with significant representation in not-so-well-known areas."[72] Yet by the 1994 meeting that clique had been broken open as the traditional "three beams" took up a smaller and smaller proportion of the papers, replaced by talks such as Pease's "Nanonatural Lithography" and life-science topics such as "Tracking Down Biological Motors Using Optical Tweezers," "Microfabricated Arrays: DNA Electrophoresis and Cell Mobility," and "Biocatalytic Synthesis of Polymers of Precisely Defined Structure."[73]

From that point on, the "lithography wars" that had characterized the conference through the 1980s receded sharply. Those wars among proponents of optical, x-ray, electron-beam, and ion-beam lithography had been waged to secure a footing in commercial semiconductor manufacturing. But optical lithography's victory, the exodus of industrial basic researchers to academia, and the growing prestige and funding associated with the life sciences made new forms of lithography, and new connections not related to semiconductor manufacturing, more attractive.

Science Policy in the Network Age

In 1992, just as the Gordon microfabrication conference series, and academic microfabrication research more generally, were gradually embracing the life sciences, the National Nanofabrication Facility's five-year grant was winding down. Recall that the NNF had been founded, under controversial circumstances, as the successor to the National Submicron Facility. The National Science Foundation had initially been reluctant to continue funding the Submicron Facility, but pressure from Cornell's industrial partners and extramural users and research collaborators helped change that decision.

Essentially the same tensions cropped up once again in 1992, but with the difference that the NNF's main rival—Stanford's Integrated Circuits Laboratory—was now interested in competing for what the NSF this time was calling the National Nanofabrication Users Facility. Under James Meindl, the ICL hadn't previously made a bid to become a national user facility, but in 1986 Meindl left Stanford to serve as provost of Rensselaer Polytechnic Institute. Meindl's successor as director of the ICL, James Plummer, worked to preserve Stanford's ability to fabricate chips on site, for the reason that doing so "will provide a great educational experience for our students, and it will enable a wide spectrum of research that would otherwise not be possible." The resources needed for on-site fabrication, however, were running low:

> The only available source of funds at present to support this activity is the CIS sponsors annual contribution. We are actively seeking external funding, but it is not an easy matter to "package" this activity in a way that is attractive to government sponsors. Everyone (CIS Sponsors included) wants to support leading edge research. To the extent that chip building is regarded as not leading edge or in direct competition with industry, it is not interesting to sponsors.[74]

The need for external support beyond that provided by the CIS industrial affiliates made NSF funding of microfabrication user facilities much more attractive than before.

Thus, the NSF's 1992 call for proposals for a National Nanofabrication Users Facility (NNUF) yielded a true competition between Cornell and Stanford. The contours of that competition were outlined in a 1991 Nanofabrication Workshop Report commissioned by the NSF and dominated by the Cornell NNF and its allies. That report made clear that the expanding disciplinary purview of micro-fabrication and nano-fabrication would have to be reflected in the expertise and equipment that NNUF would make available to users. Even so, the semiconductor industry still provided much of the justification for such a facility. As the report's conclusions put it,

> 1. Nanofabrication … underpins the electronics industry, one of the highest tech industries in the world, and also serves to stimulate other new industries. Therefore it is critical to our scientific and industrial future that the National Science Foundation (NSF) continue a strong program in this area. …
>
> 3. Nanofabrication is important to future research in electronics, materials science, and optics, and is spreading into other fields such as micromechanics and biology.
>
> 4. Based on the success of NNF research and a strong relationship to the electronics industry, NSF should continue a central facility of this type. …[75]

Although the report stopped short of identifying the biotechnology industry as one of the "other new industries" stimulated by nanofabrication, it included a long section (five pages out of thirty) on life-science applications in which the authors argued that "in some areas of the biological sciences, breakthroughs are only going to be possible through use of nanoscale devices or surfaces."

Accordingly, in the call for NNUF proposals the *second* of nine "desired facility attributes" was "creation of a stimulating in-house research environment involving several disciplines, such as engineering, physics, materials science, chemistry, biology, and biophysics. The in-house research will be expected to pursue applications of nanofabrication capabilities to a broad spectrum of engineering and scientific disciplines."[76] That requirement was a boon to Cornell, since its facility had long catered to users outside silicon microelectronics. Stanford's Integrated Circuits Lab, on the other hand, had focused much more intently on topics related to silicon microelectronics. As of 1992, all of Stanford's biomedical and life-science departments contributed less than 1 percent of the graduate students with active accounts in the ICL. By contrast, Stanford's Department of Electrical Engineering alone contributed nearly half of the ICL's active graduate student users, and the departments of Electrical Engineering, Materials Science and Engineering, Applied Physics, and Physics contributed almost 90 percent of student users.[77]

Thus, in drafting his proposal Plummer reminded his collaborators that

> NSF is looking for a facility that can support a broad range of research, including silicon, compound semiconductors, micromachining, sensors, etc. The facility will also have to serve a broad range of users—EEs [electrical engineers] working on advanced device structures, materials scientists working on new materials, biologists working on substrates on which to grow cells, etc.[78]

To meet the NSF's demands, Plummer pushed his team for more "discussion of how we're going to meet the needs of other [i.e., non-silicon] communities. Particularly the compound semiconductor guys, but also biomedical, biology, etc." Still, Plummer was confident that the ICL's strategy for serving both local and remote users would give Stanford a leg up on Cornell.

What happened next is murky and a subject of much gossip. It appears that, once again, Cornell's powerful supporters were able to keep it from losing; but Stanford's equally powerful supporters and the originality of its proposal kept Cornell from winning outright. Caught in a bind, the NSF withdrew the NNUF competition and hastily announced a new contest for a National Nanofabrication Users Network (NNUN) made up of a small, geographically distributed group of facilities.

The substitution of a national network for a national facility should be interpreted in several ways. First, it probably was an unplanned reaction to the reality that the NSF was reluctant to let either Cornell or Stanford lose the single-facility competition. As one of the external reviewers of the NNUN proposals put it, "while the proposals are excellent, the NSF solicitation on which they are based is poor. The formation of a 'network' should more properly be termed a 'dispersal' of expertise and technology in an area in which concentration of both capital and expertise are of ever increasing importance."[79]

Yet the network idea should also be seen as a creative way to fulfill the NSF's desire to support both the kind of nanofabrication research inspired by the semiconductor industry and nanofabrication's interdisciplinary, life-sciences-oriented variety. In its site-visit review, the first two questions that the NSF representatives put to leaders of the competing proposals were these:

> [1.] Specifically, what biologically-relevant projects will you target in future years?
> [2.] How do you propose to increase research in non-electronic areas such as condensed matter physics, materials, and chemistry through your proposed nanofabrication network?[80]

It is perhaps not surprising, then, that the NSF pressured Cornell and Stanford to come to some accommodation, since the need to fund both sites and the desire to support the specializations of both sites (silicon and non-silicon nanofabrication) had been motivations for widening the competition from a facility to a network in the first place. Cornell had begun negotiations with MIT, but strong hints from the NSF led Cornell to abandon MIT and partner with Stanford instead. MIT was left scrambling to assemble a network in partnership with the University of Illinois.

Still, even if the NSF would have to scrap its single-facility competition in response to intellectual and political conditions, why did it put forward a network as its alternative? Presumably the NSF could have decided instead to fund both Cornell and Stanford separately, or could have held a competition for a bilateral partnership between two nanofabrication user facilities. Instead, the NSF ended up supporting a five-campus network led by Cornell, "co-led" by Stanford, with participation by the University of California Santa Barbara, Pennsylvania State University, and Howard University. Its stated purpose was to "greatly expand access to technology [of nanofabrication equipment] across the United States," though the geographical limits of the funded network—two facilities in California and three in the Mid-Atlantic states, with none in the Midwest, Southeast, Southwest, Northwest, or New England—make that rationale rather suspect.[81]

Instead, I would argue that the network concept should be seen as inspired by the consortium mania that swept the semiconductor industry (and its state sponsors) in the 1980s and by a network mania that took root in the NSF at roughly the same time. On the one hand, the solicitation for the NNUN sounds quite a bit like earlier calls for the establishment of semiconductor research consortia:

> the vision of the Foundation [is] that the individual facilities comprising the network will work not as independent entities competing for limited resources, but rather as elements of a cooperative, integrated user facility system that seeks funding from government and industry for the common good of the network users, and allocates resources in a manner that best benefits the user community.[82]

On the other hand, the NNUN call was issued at the end of a decade in which the idea of "networks" had become integral to the NSF's operation and reputation—again, in no small part because of Moore's Law and the concomitant increase in computing power.

Most visibly, in the 1980s the Foundation funded construction of a national computing network linking universities with NSF-funded supercomputer centers—the NSFNET. Over time, the NSFNET was connected to a variety of smaller regional computer networks. That conglomeration eventually led to the NSF's designation as the federal agency that would merge and (in 1995) privatize federally supported computer networks to form the kernel of the US portion of the modern Internet. As Jason Gallo has argued, the NSF's success in helping to build, and especially in privatizing, the Internet conferred enormous influence on the Foundation in the 1990s, and made the network concept an *idée fixe* of US science policy.[83]

The 1980s were also the years in which center programs proliferated within the NSF. As we saw in chapter 4, the NSF's inheriting of the Materials Research Laboratories program from DARPA in the early 1970s helped stimulate the founding of Cornell's Submicron Facility, and the Submicron Facility plus the MRL program helped give rise to the Engineering Research Centers program, the Science and Technology Centers program, and a variety of other center-focused funding streams in the 1980s and the 1990s. Thus, at the time of the NNUN solicitation, the NSF was already familiar with the idea of coordinated *funding* of a *group* of facilities. In that sense, the NNUN represented a natural next step in experimenting with the idea of coordinated *operation* of a *network* of facilities.

The NSF's decision to support a nanofabrication network should also be seen in light of heightened interest among US policy makers and policy-oriented social scientists in the network concept as an alternative to other forms of organization, particularly markets and top-down hierarchies. Joel Podolny and Karen Page put it this way in a 1998 review: "Over the past decade or so, sociological interest in network forms of organization has blossomed. … Part of the interest in these alternative organizational arrangements is no doubt due to what some regard as their increased empirical prevalence."[84]

And which new, increasingly prevalent networks most provoked sociological interest? Semiconductor consortia in the United States and in Japan, of course: "[C]hanges in the US regulatory environment greatly facilitated the ability of US firms to engage in cooperative activities with their market competitors … [inspired by] the worldwide competitive success of Japanese … firms [which] seemed to rely extensively on network forms of organization."[85]

Thus, in the early 1990s NSF executives probably were fed a steady stream of network talk from policy makers, from corporate interests, and from the social science disciplines that the NSF itself supports. The NSF was also primed to embrace the network concept by major science policy events that shook its peer agencies in the early 1990s. Most notably, the period immediately before formation of the NNUN saw the collapse of funding for the Superconducting Supercollider (SSC) and the launch of the Human Genome Project. As Daniel Kevles has shown, the supercollider's single-site, hierarchical organization was integral to its legislative defeat, whereas the genome project's multi-site, decentralized network, spread across many congressional districts, explains much of its political success.[86] A direct line between these projects' fates and the NNUN competition can't be drawn, but no doubt NSF managers would have noticed the parallels between the single-site National Nanofabrication User Facility and the SSC, and between the multi-site NNUN and the Human Genome Project. More concretely, there is evidence that California's congressional delegation complained about the amount of NSF funding going to Cornell, much as the SSC's opponents complained about the wealth it brought to Texas—a political problem that the NNUN would have helped solve.[87]

Whatever the origins of the NNUN concept, its implementation involved a mix of centripetal and centrifugal forces that was typical of geographically distributed systems of facilities.[88] Centripetally, the facilities shared an advisory board and faced pressure from the NSF and other constituents to harmonize practices such as user fees. Centrifugally, nanofabrication was becoming a diverse enough set of practices in the 1990s that each facility could specialize in its own slice of the field: Stanford in silicon, Cornell in catering to a broad external user base, Howard in wide-band-gap semiconductors, Penn State in novel materials, and UC Santa Barbara in semiconductor etching and optoelectronics.

As the 1990s progressed, campus nanofabrication facilities continued to be founded across the United States (and around the world) and to carve out their own niche applications for the growing number of fabrication techniques. In many cases, these *ad hoc* facilities coalesced out of tool-sharing arrangements among a cluster of local faculty members. In view of the expense of such tools, though, many such facilities began to seek outside users or obtained state and federal funding that came with the expectation of access by external users. At the same time, the NNUN offered hope that the growing number of academic nanofabrication facilities could be successfully coordinated through a truly national (rather than bi-regional) network of facilities.

Thus, in the early 2000s, when the NNUN's ten-year grant wound down, the NSF decided to replace it with a much larger and more diverse National Nanotechnology Infrastructure Network. The NSF's leading role in the newly founded National Nanotechnology Initiative and its continuing reliance on the center program funding concept made enlarging the NNUN and aligning it with the NNI's objectives a natural step. When the NNIN grant came to an end, ten years later, it evolved and expanded once more—this time into the sixteen-site National Nanotechnology Coordinated Infrastructure. During the NNIN's run, its fourteen facilities pushed well beyond the aims originally articulated for the NRRFSS and the ICL in the 1970s. The Santa Barbara and Texas facilities became known for their compound semiconductor expertise, the Harvard facility handled the increasing demand for information technology relevant to nanofabrication, the Penn State facility maintained a close connection to a clean room technician training program, and so on. Five of the NNIN facilities listed a life-science area as one of their main fields of expertise, and three of them (those at Georgia Tech, the University of Washington, and Washington University) were *predominantly* biomedically oriented.

By specializing, the NNIN facilities could network more effectively not just with one another, but also with other centers on their own campus. Presumably for that reason, possession of one kind of nanotechnology center or facility seemed to give universities an enormous advantage in acquiring other such centers and facilities. We saw in chapter 4 that possession of a Materials Research Laboratory helped put Cornell, Berkeley, and MIT on the short list for the National Research and Resource Facility for Submicron Structures. In turn, Cornell's possession of the NRRFSS allowed it to compete more effectively for membership in national center programs such as the NSF's network of supercomputer centers and the Semiconductor Research Corporation's Centers of Excellence.[89] Stanford, too, repeatedly used membership in one center network to gain membership in another—a model that both schools then offered to their partners in the National Nanofabrication Users Network and the National Nanotechnology Infrastructure Network. Twelve of the fourteen NNIN campuses also had a Materials Research Science and Engineering Center (MRSEC—the successor to the MRL program) and/or at least one other nano center (as categorized by the National Nanotechnology Initiative). Five schools (including Cornell and Stanford) had an NNIN site, a MRSEC, and two other NNI-defined nano centers. In addition, most NNIN sites partnered with nearby centers that didn't officially count as "nano"—e.g., synchrotron centers or supercomputer facilities.

One outcome of the dynamic of centers begetting centers was a complex topology in which localized intra-campus research networks connected in multiple ways into national, intercampus networks associated with various center programs and their partners in government and industry. At least in fields related to nanotechnology (and what field isn't?), the American scientific enterprise began to take the shape of a network of networks, with academic centers as the most-connected nodes within and between networks. Even the social sciences and the humanities have been incorporated into these meta-networks. Research for this book was supported by the NNIN's Social and Ethical Issues program, coordinated from the Cornell facility.

Despite the ever-expanding disciplinary reach of these interconnected networks, research related to commercial microelectronics, and even more narrowly to semiconductor manufacturing, still went on at the NNIN facilities and at many other academic nano centers. But the semiconductor industry was not the dominant player it once was. One aim of the NNIN was to push its sites toward self-sufficiency by attracting as many commercial users as it could, regardless of industrial sector. That meant the NNIN sites eagerly sought users from firms that did *not* have semiconductor processing equipment, and hence from firms outside (or at the margins of) semiconductor manufacturing. The NNIN sites also added other equipment that would appeal to firms in industries other than microelectronics. Thus, users increasingly came from firms in biotechnology, in medical devices, and in solar energy, and even from firms in the paper industry. Even NNIN users based in microelectronics or semiconductor firms tended to be based in small, young firms, whereas most of the industrial partners of the NRRFSS and CIS had been large, established businesses.

Thus, the NNIN and its member facilities perfectly embody several of the major post–Cold War transitions in US science and science policy: the collapsing of disciplinary hierarchies, the value placed on interdisciplinarity, the enormous prestige of the life sciences, the cultural and economic currency of start-ups and entrepreneurship (especially academic entrepreneurship), and the turn toward network forms of organization. What I have tried to show in this chapter is that the microelectronics industry was at the forefront of many of those changes, and that it was the instigator of many of the institutions that have become vehicles of those changes. Academic nanofabrication is well past the era when it was viewed as an auxiliary of the semiconductor industry, yet that industry's evolving needs have left their traces on the institutions and organizations of academic nanofabrication even as that field has greatly enlarged its scope of practices and applications.

6 Synthesis at the Center

In earlier chapters, I surveyed several research fields at the margins of the microelectronics industry—molecular electronics, Josephson computing, the academic microfabrication community—as though they were distinct lines of inquiry. And they were, mostly—there were only a few individuals who hopped among those fields. But industry observers who were trying to forecast the direction that microelectronics would take in the 1980s—journalists, public intellectuals, policy makers, etc.—often lumped these marginal activities into the same category. For instance, in reporting on Forrest Carter and Ari Aviram's work on molecular electronics in the magazine *Science* in 1983, Arthur Robinson noted that

> for all their high-tech razzle-dazzle, the semiconductor microelectronics wizards are remarkably conservative. They began with silicon and no semiconductor has come close to replacing it. Most computer designers chortle when advanced alternatives such as superconducting Josephson junction electronics are mentioned. Molecular electronics, then, is not a high-priority item, as the Navy's Carter admits. "If we had more support, we would have more results," he says. Rather than a molecular computer, a possible outcome of molecular electronics research is that in trying to bring their dreams to fruition, researchers will uncover new micro- or nanofabrication technologies with applications quite different from those they now imagine.[1]

Here Josephson computing, molecular electronics, and advanced micro/nanofabrication techniques all were covered in the same breath as alternatives to silicon, and all were described as mocked by the silicon establishment.

Similarly, when *National Geographic* surveyed Silicon Valley and the microelectronics industry in 1982, it parked all of the different organizational and technological experiments in microelectronics that I have discussed under a single umbrella. The article is worth quoting at length just to see how closely connected this book's case studies were as equally likely potential paths to a future electronics:

> Shrinking microcircuits put a premium on new tools to make chips with exquisite precision. At an IBM plant in eastern New York, beams of electrons transfer chip designs directly from computers to wafers. And they do it with an accuracy comparable to a skipper holding his ship within 525 feet of its course throughout a voyage from New York to New Orleans.
>
> Such beams have unmatched potential to pattern wafers with incredibly fine circuits. At the National Research and Resource Facility for Submicron Structures at Cornell University, Dr. Michael Isaacson has carved into salt crystals letters so tiny that a 30-volume encyclopedia could be written on a chip the size of a half-dollar.
>
> Other scientists try building chip circuits, atom by atom, of chemicals beamed at wafers. The goal of such "molecular beam epitaxy" is more transistors on chips, packed in three-dimensional rather than flat arrays. The process can also sheet wafers with layers of gallium and arsenic compounds that conduct electricity ten times as fast as silicon.
>
> The drive to cram more components on the chip may end in a test tube, says chemist Forrest L. Carter of the U. S. Naval Research Laboratory in Washington, D. C. Dr. Carter thinks that relatively soon molecule-size computer switches will be synthesized from inorganic chemicals, like some drugs. Then, within 30 years, we could be jamming a cubic centimeter "with a million billion molecular switches, more, probably, than all transistors ever made."
>
> From Bell Telephone Laboratories scientist Andrew Bobeck has come the magnetic bubble memory. On this chip, bubble-shaped magnetic areas in a film of garnet crystal store such computerized messages as, "We're sorry, but the number you have reached has been changed to. ..." One day, Bobeck told me, a bubble chip the size of a postage stamp will hold the contents of a small phone book.
>
> Researchers at Bell Labs, IBM, and elsewhere are refining Josephson junctions—electronic switches made of metals that lose all resistance to electric current when chilled to near absolute zero. Chips with these devices can switch signals in seven-trillionths of a second, presaging ultrafast telephone switching equipment, or a refrigerated supercomputer. Its chilled circuits could be packed into the volume of a grapefruit, cutting travel time for signals and enabling the machine to carry out 60 million instructions a second, ten times as many as current high-performance computers.
>
> IBM hopes to build a prototype in a few years. "Could it be of commercial significance?" IBM's Dr. [Lewis] Branscomb baited me. "I'll tell you in the 1990s."
>
> BY THEN the Japanese may have created a thinking computer. Memory-chip successes have inspired the Ministry for International Trade and Industry to launch work on a machine that may win Japan command of the technological revolution being sparked by the chip.[2]

Here too, exotic microfabrication techniques (e-beam, MBE, etc.), the NRRFSS, molecular electronics, Josephson computing, and innovations from Japan were pictured by industry observers as conjoined, emerging alternatives to conservative West Coast semiconductor manufacturing.

That entanglement of different ways to reach the microelectronic future has continued to the present day. Some of those paths—such as Josephson computing—seem more treacherous now than in 1982, but most of the rest surveyed in *National Geographic* are as plausible (or implausible) now as then. And they have continued to sit side by side well into the twenty-first century. In the United States, when the National Nanotechnology Initiative was founded in 2000, it coordinated federal support for all of the areas listed above: exotic lithographic techniques, the National Nanotechnology Infrastructure Network (descendant of the NRRFSS), molecular electronics, and quantum computing (a research area that, in some of its variants, incorporates Josephson junctions).

This chapter will trace the co-evolution of these organizational and technological experiments by braiding together two seemingly distinct—but actually multiply intersecting—case studies. The first case narrates the further adventures of the American molecular electronics community. In this case study, we will follow further the shift away from industrial basic research, the resultant uptick in the industrial aspirations of academic research (as industrial researchers moved into university positions, and as firms increasingly outsourced their basic research needs to academic groups), and the growing dominance of a government-coordinated "network failure" model of science policy bridging universities and industry. We will also return to the theme of the ambivalent utility of charisma that was broached in chapter 3. Molecular electronics' US camp continued to be fertile ground for compelling visions and persuasive storytellers through the 1990s, with mixed results for the field as a whole.

The second case study threads the story of molecular electronics into a local (but nationally and globally important) story about the use of microelectronics and late/post–Cold War science policy in academic institution building. The locus here will be the chemist Richard E. (Rick) Smalley and the diversified "garden" of organizational and technological experiments he cultivated at Rice University.

Smalley is already well known in the folk and scholarly history and sociology of science, primarily for the co-discovery of buckminsterfullerene in 1985 and his political and intellectual advocacy for nanotechnology in the 1990s and the early 2000s. What earlier studies have missed, though, is Smalley's use of both buckminsterfullerene and nanotechnology as tools to assemble exactly the kinds of organizational experiments we have surveyed in this book: academic centers, interdisciplinary collaborations, university-industry partnerships, government amelioration of network failures, and academic patenting and entrepreneurship. Not coincidentally, many of Smalley's organizational experiments were aimed at fostering research into exotic forms of electronics, especially molecular electronics. Smalley was a prominent patron and partner of some of the leading exponents of Aviram-Ratner-Carter-type molecular electronics in the 1990s, and the field he helped found—the study of new allotropes of carbon—had by the early twenty-first century become the most promising avenue to realization of something like molecular electronics.

Molecular Electronics Redux Redux

As we saw at the end of chapter 3, molecular electronics research came to a global pause at the close of the Cold War. In the United States, that was due in part to the death of Forrest Carter. In the United Kingdom and in Japan, the ending of dedicated funding streams left molecular electronics researchers looking for new patrons. And in Poland, in Hungary, and in the former Soviet Union the transition to a post-socialist political economy disrupted research fields and sparked the migration of many scientists to the West.

Despite the apparent calm, a third generation of molecular electronics was beginning to form. In the United States, that regrouping was instigated in large part by a newly visible Ari Aviram. The impetus for that third generation, and for Aviram's re-emergence as a leader in the field, was partly technological. As we saw in chapters 4 and 5, by the late 1980s micro/nanofabrication techniques had progressed and diversified to the point where it was now possible to interrogate the electronic properties of a single molecule. In Aviram's case, the scanning tunneling microscope (STM)—invented at IBM in 1981–82—opened up possibilities for molecular electronics that hadn't existed in the 1970s.

The STM works by bringing a very sharp metal or semiconductor probe close enough to a sample that measurable numbers of electrons begin to quantum mechanically "tunnel" between probe and sample. The first generation of STMs were used to characterize metal and semiconductor surfaces, but by the late 1980s IBM researchers were pursuing STM characterization of single organic molecules such as strands of DNA, individual DNA bases (guanine, cytosine, etc.), liquid crystal molecules, and carbon monoxide. IBM strongly encouraged its researchers to take up the STM, to the extent of essentially giving away STMs to groups at the Yorktown lab.[3] Aviram, therefore, was able to leverage the firm's support for the STM as a means of pursuing his old idea for a molecular rectifier. By 1988, he and two IBM colleagues, Christian Joachim and Melvin Pomerantz, were able to report "evidence of switching and rectification by a single molecule effected with a scanning tunneling microscope."[4]

At the same time, Aviram began to step into the institution-building role formerly occupied by Forrest Carter. In February of 1989 he held a conference in Hawaii on "Molecular Electronics: Science and Technology," with support from the Engineering Foundation. In the next fifteen years, the Engineering Foundation supported five more such meetings, making them the institutional mainstay of the third generation of molecular electronics in the United States. Indeed, thanks to Aviram's inclusion of representatives from the United Kingdom, continental Europe, and Japan, the Engineering Foundation conferences integrated the global molecular electronics community more tightly than Carter's meetings had.

Aviram also shaped the Engineering Foundation meetings to redress the field's dubious reputation in the United States. As he put it in a post-conference report on the 1989 meeting, "though the name of the field [of molecular electronics] implies technological applications, the state of the art is at a stage that requires considerable basic science groundwork to foster a solid foundation for building future technology."[5] The conference proceedings therefore emphasized reports on current, experimental progress much more than Carter's had, while limiting the far-out speculation that Carter had indulged. Notably, Aviram invited IBM's Robert Keyes to give extremely skeptical talks at both the 1989 and 1991 meetings that some attendees remember as putting a heavy damper on the audience's hopes for molecular computing. Keyes put it this way in the published version of his 1989 talk:

> In spite of the very favorable properties of transistors and of the potential for much more miniaturization of them, there has been a continuous interest in other types of devices that might perform logic operations. A few have been the subject of major development efforts. However, none has been able to match the high performance of semiconductor transistors. … Even if other kinds of devices can be made smaller than semiconductor devices, it is doubtful that they can be taken seriously as replacements for transistors in most applications. … [Physical constraints will] limit the size of transistors. Various other kinds of devices may avoid this limit, but they do not have the high gain, tolerance of variability, and good input-output isolation of transistors, and are not likely to compete with transistors in digital applications.[6]

In other words, it might be possible for molecular electronics to leap-frog the miniaturization aspect of Moore's Law—but remember that Moore's

Law is about the economics and manufacturability of integrated circuits, and not just about the miniaturization of circuits. Keyes argued that, in terms of economics and technological utility, molecular electronics would always fall short, as had Josephson junctions before it.

Some molecular electronics researchers saw an escape hatch from Keyes' condemnation, though. If organic molecular components could somehow be integrated with a semiconductor architecture, then perhaps they could co-opt silicon's advantages rather than trying to overcome them. Accordingly, Aviram extended Carter's earlier efforts to bring microfabrication specialists into the conversation about molecular electronics. At his 1989 meeting, for instance, Henry Smith of MIT's Submicron Structures Laboratory gave a talk on x-ray nanolithography and Fabian Pease of Stanford spoke on e-beam resists.

From ULTRA to Moletronics

Aviram's Engineering Foundation conferences may have had their most lasting impact in—almost accidentally—fostering one particular collaboration between a microfabrication expert and an organic chemist. The microfabrication specialist was Mark Reed, an applied physicist whom Aviram invited to his 1991 Engineering Foundation conference in the US Virgin Islands. Reed was presumably invited because he was among the first to fabricate so-called "quantum dots"—nanocrystals in which electrons and holes are so spatially confined that their de Broglie wavelengths are restricted to discrete values rather than the smeared density of states seen in bulk crystals. Reed's quantum-dot work took place at Texas Instruments, but in 1990 he moved to Yale University and joined the ranks of corporate physical and engineering scientists who left the shrinking world of corporate research at the end of the Cold War. Thus, when he received Aviram's invitation he was in the middle of transitioning to a new job and perhaps on the lookout for new applications of his microfabrication expertise. Yet his initial reaction to the invitation was negative, given the speculative character of American molecular electronics that he had witnessed at one of Forrest Carter's workshops. He was swayed to attend Aviram's meeting only because the Caribbean venue would allow him to indulge a favorite pastime—scuba diving.

One of the conference talks jumped out at him, though—a paper by a young synthetic organic chemist at the University of South Carolina named James Tour about his recent attempts to synthesize an Aviram-type molecular rectifier. Although he was still a long way from showing that one of his molecules had the current-voltage characteristics of a rectifier, Tour's ability to execute very difficult syntheses, and to speak persuasively about potential applications for the molecules he synthesized, was already evident. As Reed listened to Tour's description of how he planned to synthesize molecular circuit components, it occurred to him that microfabrication techniques might provide a way to address and interrogate those components—in other words, to actually incorporate them into a circuit, albeit initially a circuit useful only for scientific experiments, not high-speed computing.

As it turned out, Reed and Tour were already slightly acquainted—when Reed was a graduate student at Syracuse University he had been a teaching fellow in a course Tour had taken as an undergraduate. They didn't figure that out until later, but it may have been in the background as Reed persuaded Tour to co-author a white paper outlining their ideas in order to secure funding. Eventually, the white paper fell into the hands of a DARPA executive (later deputy director), Jane "Xan" Alexander, who was putting together a microelectronics program called ULTRA—for "ultrafast, ultra-dense computing components."[7]

Mark Reed was exactly the kind of person ULTRA was designed to fund: an academic microfabrication specialist familiar (from his time at TI) with linking basic research both to commercial objectives and to long-range military needs. Jim Tour and molecular electronics, however, were outliers for ULTRA. Yet moderately plausible outliers are precisely what DARPA officers are supposed to look for. In the US military research funding ecology, conventional projects are funded by the individual services' R&D arms—the Office of Naval Research, the Air Force Office of Sponsored Research, the Army Research Office—while long-range, unconventional, high-risk projects fall within DARPA's purview. It was in that light that Alexander attached the Reed-Tour collaboration—and molecular electronics more generally—to the semiconductor-oriented ULTRA program.

For the next few years, then, Reed and Tour enjoyed generous funding from DARPA which they used to microfabricate structures to test the electrical characteristics of Aviram-type molecules. The basic aim that they and a few other molecular electronics researchers were pursuing was to make two closely spaced electrodes and then to place a single molecule across those electrodes such that an electrical current could flow between them and complete a circuit. One could imagine several different ways to achieve that. A molecule resting on a flat substrate with an STM tip hovering over it is one, though in that case the electrons tunnel between tip and molecule rather than move as in a wire. Alternatively, it might be possible to fabricate

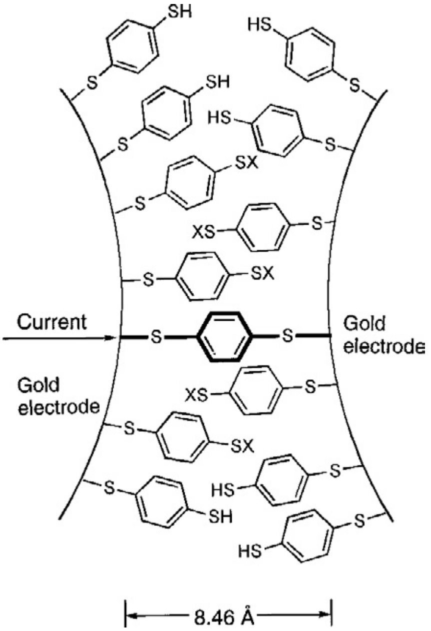


Figure 6.1
A Reed-Tour-type break junction. From M. A. Reed, C. Zhou, C. J. Muller, T. P. Burgin, and J. M. Tour, "Conductance of a Molecular Junction," *Science* 278 (1997): 252–254. Reprinted with permission from AAAS.

(or move) two electrodes very close to each other, or to take a wire and etch out a gap leaving behind two electrodes.

The route Reed and Tour eventually happened on is called a "break junction" (figure 6.1). As the name implies, this involves etching a small bridge connecting two larger plates and then physically flexing the bridge just enough that it cracks. When the bridge is relaxed again there is a molecule-sized gap between the two ends of the bridge, into which can be poured or diffused enough Aviram-type molecules that one of them will, it is hoped, lie across the two ends in exactly the right way. The two plates are then connected to a conventional circuit and current flows through the molecule.

The ULTRA program gave Reed and Tour the resources to progress toward that goal. In return, they introduced other ULTRA participants to a compelling vision of the molecular future, while preparing the ground for other molecular electronics researchers to link into the DARPA network. To accomplish that double sell, Reed and Tour took two complementary rhetorical tacks. Reed, with a pedigree and expertise more familiar to silicon specialists, more often (though not always) handled the task of lowering expectations for molecular electronics. Tour, by contrast, seems to have relished the role of visionary and popularizer, unabashed both in generating media attention and in making rosy predictions that a Pentium chip in a test tube was just around the corner.

Wired put it this way in an article titled "Molecular Electronics Will Change Everything":

> Once again, Jim Tour has forgotten to breathe. … Tour's specialty is inventing molecules with novel attributes, but it's up to his research partner, a soft-spoken electrical engineer named Mark Reed, to test them. … Tour is a natural stage presence, a man of faith who not only was denied a career in law enforcement but resisted an instinct for missionary work. Reed is genial, casual.[8]

That division of labor—both rhetorical and experimental—was promising enough that as Reed and Tour's initial five-year grant wound down, DARPA decided to spin off a new program called Moletronics. For the first time in the United States, molecular electronics—as a field rather than as isolated efforts—had the support of a major funding agency.

Molecular Electronics' Gilded Age

With the Moletronics program, American molecular electronics entered an era of dazzlingly fast progress and even more dazzling publicity. But by the early 2000s it had become apparent to many inside and outside the field that progress had been so fast that mistakes had been made, and the publicity so favorable that those mistakes hadn't been reckoned with. Those conditions set the field up for infighting, embarrassment, and reconsideration of its aims and means of achieving them.

That was not, of course, DARPA's intent. The Moletronics program was designed to ameliorate network failures—failures of the type that Philipp Brandt and Josh Whitford pithily summarize as the belief by one participant that another has either "screwed up" or "screwed us."[9] That is, Moletronics was organized not simply as a funding source doling out money to isolated groups, but as a network of collaborations where DARPA encouraged participants to get to know (and perhaps trust) one another—and to know (and perhaps trust) one another's capabilities. The Moletronics approach was similar to that of other DARPA advanced electronics initiatives of the 1990s. As a grant officer associated with one of those programs told me,

> the culture, especially of most university folks at that time, is that "it's my lab and I have my space." Yes they go to conferences and talk to each other, but they

> each have their own project. Whereas I had been picking people because I could actually see my other groups having a problem six months from now, and this group can provide the answer. They don't know that yet, and I would introduce them about a year before they needed to talk to each other. I did that by telling them we're all having lunch together. There was a case where it was the two year mark when somebody asked why I didn't introduce them to someone sooner. I said, "do you remember [that meeting last year]? Do you remember lunch? Oh yes, I did introduce you." They just weren't ready to talk, but at least they started to hear about each other's work.[10]

About half a dozen collaborative efforts formed the core of the Moletronics network.[11] We will encounter a few of them again as we interleave this chapter's other case study of nanotechnology at Rice University. One was associated with the reentrant Mark Ratner, now at Northwestern University, and a growing array of collaborators who would eventually form Northwestern's Institute for Nanotechnology. Another was a "team led by Charles Lieber at Harvard University [which] started out with the goal of exploiting the novel electronic and mechanical properties of carbon nanotubes for computation" but later widened its focus to non-carbon nanowires.[12] Carbon nanotubes were at the center of Rick Smalley's nanotechnology efforts at Rice, and at one point he attempted to form a dispersed "center" with Lieber.

The two groups we'll examine most closely were widely regarded as the leading efforts in the Moletronics program—or, more objectively, were by a wide margin the two efforts that garnered the most media attention. They were also, not coincidentally, widely seen as rivals. Some participants speak of a West Coast–East Coast contrast in styles that mimicked the contemporaneous coastal rivalry in hip-hop. In that analogy, the "West Coast" team was a collaborative effort between Hewlett-Packard (based in Silicon Valley) and the University of California Los Angeles. The leader of the Hewlett-Packard team, Stan Williams, had been an undergraduate at Rice University, and was an advisor to Rice's nascent nanotechnology activities in the 1990s. Williams' main UCLA collaborator, Jim Heath, had been Rick Smalley's PhD student.

The "East Coast," meanwhile, was represented by Reed and Tour, who were now joined by two collaborators from Pennsylvania State University—David Allara, a surface scientist, and Paul Weiss, an expert in scanning tunneling microscopy and related techniques. Two other Penn State researchers—Teresa Mayer and Tom Mallouk—also formed a node in the Moletronics network and intermittently worked with the Reed-Tour-Allara-Weiss collaboration. Here the connection to our next case study came primarily through Jim Tour, whom Rick Smalley had recruited to Rice to build up that school's profile in molecular electronics. A secondary connection passed through Kevin Kelly, a Rice graduate student who then took a postdoctoral fellowship with Weiss before returning to Rice as a faculty member and a collaborator with Tour.

In Schrank and Whitford's terminology, the source of friction between the HP-UCLA and Yale-Rice-PSU groups derived from the fact that both groups did "screw up," leading to rounds of mutual doubt-casting in which most participants could feel that at least some of the others had "screwed us." To my eye, neither group's missteps were egregious, and would not have merited comment in a less fevered atmosphere. But the atmosphere *was* fevered; fairly ordinary mistakes were amplified precisely because both groups had been so publicly optimistic not just about their laboratory experiments but also about the short-term commercial potential of their ideas. Despite the field's reboot under DARPA sponsorship, the American version of molecular electronics continued to suffer from, and perhaps even merit, a louche reputation. Indeed, Robert Metzger—one of the few researchers to bridge between the 1980s' and the 1990s' iterations of molecular electronics—assessed DARPA's network building as little different from Carter's:

> In the early 1980s, three topical conferences organized by the late Forrest L. Carter galvanized some interest and, alas, some vacuous press-agentry. In the late 1990s, the United States Defense Advanced Research Projects Agency sponsored attempts towards molecular-integrated circuits and UE [unimolecular electronics]; this also generated some press-agentry.[13]

Both teams did make real discoveries, but not always quite the discoveries they initially claimed and used to justify rosy commercial projections. Note, for instance, the ambivalent success of the HP-UCLA team. Their approach, roughly, was to pattern stripes of metal or semiconductor on a substrate, then deposit a layer of switchable molecules (molecules that could be flipped from one configuration to another), and then to attach the free end of the molecules to patterned stripes on a second metal or semiconductor substrate. (Figure 6.2 shows a schematic of this idea, created by *Science*, which the HP-UCLA team complained didn't convey the subtleties of their approach.) The top and bottom substrate stripes were orthogonal to each other; therefore, if one assigned a number to each stripe, every switchable molecule would have a unique two-digit address—say (3-top, 5-bottom) or (4-top, 1-bottom). Each molecule could therefore store a single addressable bit.

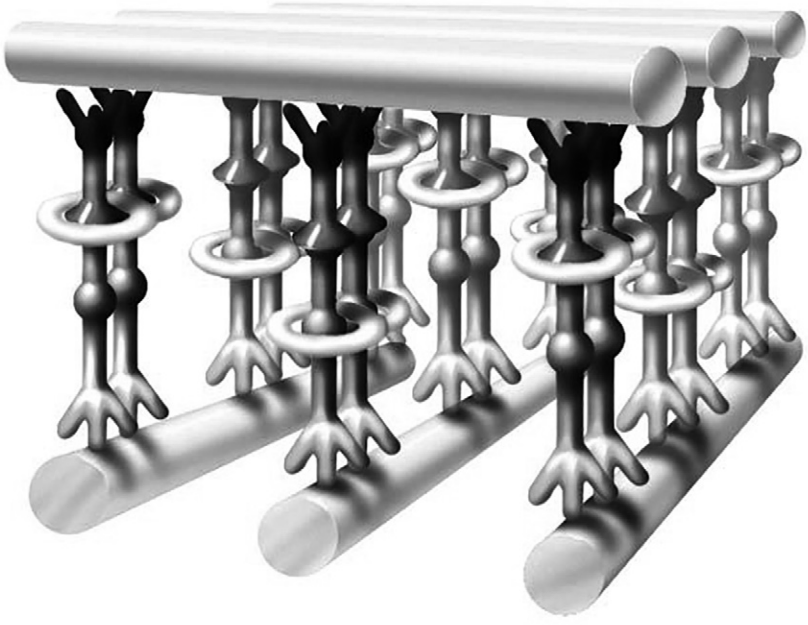


Figure 6.2
An HP-UCLA-type rotaxane array, as portrayed in *Science*. Illustration by C. Slayden reproduced from Robert F. Service, “Next-Generation Technology Hits an Early Midlife Crisis,” *Science* 302 (2003): 556–559; reprinted with permission from AAAS. The same image appeared in a few of Service’s other news articles on molecular electronics, though after the “Midlife Crisis” article the HP-UCLA team disputed that it was an accurate rendering of their system.

In 1999, the HP-UCLA team announced in *Science* that “logic gates were fabricated from an array of configurable switches, each consisting of a monolayer of redox-active rotaxanes sandwiched between metal electrodes.”[14] That finding generated lavish attention in the media, such as a *New York Times* article heralding Heath and Williams as ushering in “a renaissance in computer science and design.”[15] Some of Heath and Williams’ colleagues, however, criticized their interpretation. As *Science* put it, “models like this one [figure 6.2] for rotaxanes are ‘somewhere between naïve and misleading,’ Paul Weiss says.”[16] Weiss didn’t criticize Heath and Williams by name, but all readers would have known who he meant. As Heath later put it, the period was characterized by “quite a bit of confusion (and controversy) that took some time to resolve.”[17] Eventually, the Hewlett-Packard group showed the “switching response to be molecule independent. … In fact, the molecular film could be replaced with an inorganic insulator film, and similar switching response occurred."[18] This was novel behavior which Hewlett-Packard and other firms are beginning to commercialize—but not quite "molecular" electronics, since it was the inorganic matrix, not the organic molecular components, that mattered.

The same *Science* article that quoted Paul Weiss' oblique criticism of the HP-UCLA team—an article titled "Next-Generation Technology Hits an Early Midlife Crisis"—also offered his skeptical view of findings from his own Yale–Rice–Penn State team. In a 1999 article in *Science*, Reed and Tour's groups reported having obtained successful "conductance of a molecular junction" using the break-junction method.[19] Here too, however, it soon became clear that what was actually going on between the two electrodes might be different than the naïve model suggested. There might, for instance, be more than one molecule lying between the electrodes, or two molecules might be attached to two different electrodes but lying such that their electron orbitals overlapped. Again, *Science* profiled Weiss as an inside critic of the Yale-Penn-PSU team: "There are a number of parts of the field that really weren't critically tested before being publicized and published. So people are starting to backpedal." That sentiment was shared by external observers.

Suspicions about the initial Yale–Rice–Penn State results eventually forced the team's leaders to abandon the break-junction set-up in favor of alternative means (such as microfabricated "nanopores") for holding a molecule stationary between two electrodes. Despite the controversy, though, Reed, Tour, Mallouk, Allara, and Brosl Hasslacher (a theorist at Los Alamos National Laboratory) used the break-junction data as the grounds for founding a company, the Molecular Electronics Corporation, in 1999.[20] At the same time, Reed and Tour filed applications for an extremely ambitious and visionary set of patents, most notably one (filed with Weiss, Allara, and one of Tour's former postdocs) for a "molecular computer."[21] The fact that that patent would expire in 2020 conveyed to investors, colleagues, and the media a sense that molecular electronics was truly on the cusp of commercialization.

And yet, despite glowing publicity—as *Penn State News* put it in 2001, "stories about the company and its founders have appeared in *Scientific American*, *Wired*, the *New York Times*, and the *Economist*: pretty heady stuff for a place that only recently expanded its payroll to 11 people"—the Molecular Electronics Corporation never attracted enough customers or venture capital to keep going.[22] Some of its misfortunes, as we'll see, were beyond its founders' control. But their optimistic predictions and experimental reports encouraged other molecular electronics researchers to (sometimes incautiously) keep pace with their optimism, while providing opportunities for molecular electronics' critics to cast doubt on the field as a whole. As Edwin Chandross—Forrest Carter's most scorching critic in the 1980s—put it in a letter to *Science* criticizing the HP-UCLA and Yale–Penn State–Rice teams in 2004, "The proponents of this 'technology' have buried us in hype for several years. … Such unreasonable advertising should have aroused the skepticism even of novices."[23]

All this is not meant as a condemnation of either the HP-UCLA group or the Yale–Penn State–Rice group. I should note that my own position on the faculty of Rice University enabled me to become acquainted with several members of both teams—quite well with some of them. In fact, I had met five of the principals—Reed, Tour, Weiss, Williams, and Allara—before moving to Rice. There is no such thing as value-free history of science, and in my case one of those values is that I like and admire these people. But neither is there value-free science. My point in this section has been that the "long arm of Moore's Law" inserted a tissue of values into molecular electronics research that was occasionally at odds with some of the other values that ostensibly frame modern science, such as the conservative reporting of experimental results obtained in the curiosity-driven quest for fundamental knowledge. And yet that quest for fundamental knowledge would not be sustainable without either the resources provided by Moore's Law's long arm or the optimism that Moore's Law can be extended via as-yet-undiscovered means.

The Reactivity of Small Clusters

After the above admission, I hope readers will now allow me to turn fully to Rice University and to Jim Tour's more famous and even more visionary colleague Rick Smalley. Lest readers think that my interest in Smalley is parochial, I would point out that since his death in 2005 Smalley has become the focus of something like a cottage industry in the history and sociology of science, with a spate of studies of his role in the founding—and the survival—of the US National Nanotechnology Initiative.[24] Smalley's role in the discovery of buckminsterfullerene, for which he shared a third of the 1996 Nobel Prize in chemistry, has also been the subject of several popular histories.[25] Two years after his death, a scientometric study rated Smalley the most-cited author (of 401 listed) in the entire nanotechnology literature.[26]

More germane to this book—symbolically at least—is that in 2003 Smalley was the only academic scientist invited to the Oval Office for the signing of the 21st Century Nanotechnology Research and Development Act. As Patrick McCray has shown, that act—and the National Nanotechnology Initiative, which it supported—were established in no small measure in order to aid the US semiconductor industry in keeping up with overseas competition.[27] The NNI and its enabling legislation were perhaps the ultimate expression of the long arm of Moore's law. And for the leaders of the NNI, Rick Smalley embodied the kind of academic scientist—ex-industrial researcher, government adviser, entrepreneur, center builder, science popularizer—that the United States needed in order to respond to the challenge of extending Moore's Law.

Expectations for Smalley were high from the moment he arrived at Rice in 1976, as was evident in his rise from assistant to associate professor in 1980, to full professor in 1981, and in 1982 to an endowed chair named after the president of Rice, Norman Hackerman, and his wife. Smalley had been brought to Rice on the strength of his work in laser spectroscopy, an area that several Rice faculty members already specialized in. In 1979, a few of these senior faculty members, primarily from the departments of Chemistry, Electrical Engineering, and Physics, founded the Rice Quantum Institute (RQI) to foster interdisciplinary research in the field of laser spectroscopy. As we saw in chapter 1, the political and economic turmoil of the era constrained, but also encouraged, organizational experiments of this sort. As the RQI's founders put it in 1995,

> At the time … universities across the nation were recovering from very substantial cuts in Federal spending for research (a combination of cutbacks in the space program and the debt and disenchantment with technology attendant to the aftermath of the Vietnam conflict); there was essentially a freeze on faculty hiring, and it was not appropriate to extend our structure at Rice. We did, however, decide to see what we could do with "just a name"; so we "got together and called ourselves an institute."[28]

Smalley was not deeply involved in the formation of the RQI, but he was drawn in as a founding member and his work with other Rice professors was seen as a model of the kind of interdisciplinary collaborations the RQI would foster.

For its first few years the RQI was a low-key affair. Although individuals and small teams of RQI members succeeded in gaining funding, none of the external funding proposals for the RQI as a whole were funded. Then in 1985 a series of events changed both Smalley and the RQI dramatically.

That summer, Smalley, Bob Curl (one of the founders of the RQI), and Harry Kroto (from the University of Sussex)—along with three graduate students—discovered C_{60}, the first of a class of molecules, known as buckminsterfullerenes, that formed the third allotrope of pure carbon (after diamond and graphite).

The Kroto-Curl-Smalley collaboration came about rather fortuitously. As a postdoc at the University of Chicago, Smalley had invented an apparatus for making spectroscopic measurements of very small, cold clusters of atoms. A version of that device which he and his students built at Rice—known as the AP2—formed the basis for his collaborations with Curl and other members of the RQI. When Kroto encountered Curl at a conference in the early 1980s, the AP2 came up in conversation, and Kroto seized on it as the means to investigate the chemical makeup of matter in interstellar space—an environment he believed contained very cold, small clusters of carbon atoms which he thought could be simulated in the AP2. In the late summer of 1985, Smalley finally agreed to generate carbon clusters with the AP2, Kroto flew to Houston, and over the next few days Curl, Kroto, Smalley, and their graduate students stumbled upon, then struggled to interpret, data indicating the presence of a molecule made up of sixty carbon atoms forming a closed cage—C_{60}.

Kroto's astrochemistry aims for the C_{60} experiment are relatively well known. Usually left out of the C_{60} story is any sense of what Smalley was doing with the AP2 to begin with. And here the long arm of Moore's Law begins to intrude. For more than a year, Smalley was reluctant to follow Kroto's suggestion for an AP2 carbon experiment. There were two reasons. First, some colleagues at Exxon's research lab had already looked at carbon clusters in a version of the AP2 that Smalley had built for them and had found nothing significant. Before going to graduate school at Princeton, Smalley had worked for Shell, and throughout his career he maintained close ties with the energy industry. Smalley would later fall out with the Exxon team, but in 1985 he still trusted them enough that he didn't want to replicate (or be seen to doubt) one of their experiments. Second, and more important for our story, Smalley's current project at the time Kroto first suggested examining carbon was a study of the reactivity of small clusters (today we would call them nanoclusters) of *semiconductor* materials. The AP2 was originally built to study metal clusters, but in the months before C_{60} was discovered Smalley's group had moved on to the question of whether semiconductor clusters differ significantly from metal ones. (They do.) Because the properties of semiconductors can vary wildly in the presence of even minute amounts of impurities, Smalley was hesitant to put a carbon disc into the AP2 until the runs with silicon, germanium, and gallium arsenide had come to a pause.

The AP2 semiconductor cluster experiments were, in a sense, the kind of incremental basic research that scientists sometimes describe as "picking the low-hanging fruit." Smalley had an experimental apparatus, and the periodic table provided a menu of elements and simple compounds to put in it. Semiconductor clusters, in that light, were epistemically no different from the metal clusters they followed or the carbon clusters they preceded. But epistemology is one thing; gaining the resources to do an actual experiment, and then using that experiment to gain further resources—what Bruno Latour and Steve Woolgar referred to as the cycle of credit—is another.[29] And on that score, semiconductor clusters were decidedly different from metal ones.

That is, semiconductor research has a large, well-resourced audience with specific technological aims in mind, to which Smalley et al. played in setting up their experiments with semiconductor nanoclusters:

> Driven by the extreme technological importance of new breeds of semiconducting materials, there has been quite an active interest in theoretical models of III-V semiconductors. … Virtually all theoretical approaches to semiconductor surfaces and interfaces start with a relatively small cluster of atoms and … compare to bulk surface measurements. … There is still a potentially severe mismatch between the essentially microscopic theory and essentially macroscopic experiments. One appealing way out … is … by developing techniques for generating and probing the very clusters the theory is best able to handle. Certainly this will not be a universal solution. Particularly for semiconductors (where the major device-driven interest often focuses on such intrinsically macroscopic phenomena as depletion layers, etc.), not all properties of bulk interfaces will be accessible through the study of microscopic clusters. But the crucial short-range phenomena … occur in the small clusters as well.[30]

Notably, the large discs of silicon, germanium, and gallium arsenide used in these experiments came from Texas Instruments, and one of the PhD students working on the project, Sean O'Brien, went to work for TI after he left Rice.

The other graduate student who, with O'Brien, was involved in the most intensive parts of the discovery of C_{60} was Jim Heath. (Another grad student, Yuan Liu, participated in the initial phase of the carbon experiment before going on vacation.) Both O'Brien and Heath stayed on at Rice as postdocs, tasked largely with finding a way to make sufficient amounts of C_{60} so that the new material could be subjected to bulk spectroscopy. Their—and Smalley's—work on semiconductor clusters was put on a back burner then, and is forgotten in the popular and scholarly historiography today. That is no wonder, since Smalley's most-cited semiconductor cluster article has received fewer than 1 percent of the citations of the article announcing C_{60}. Still, semiconductor materials—and semiconductor companies such as TI—formed the context for Smalley's contribution to the discovery of C_{60}.

An Institute Tries to Become a Center

Although full acceptance of the claims about C_{60} would not come until five years later, the announcement of the Rice-Sussex discovery in 1985 was greeted with wide acclaim. That accelerated other universities' attempts to lure Richard Smalley away from Rice. It quickly became obvious that Rice University would have to build a significant program around Smalley to keep him in Houston. Action could not be taken immediately, though, because at just this point Rice's provost retired. Perhaps as a preemptive bid to keep Smalley happy, he was named chairman of the committee to find a new provost—an administrator who would be in a position to facilitate or hinder his research ambitions.

Eventually the committee chose a Rice physicist, Neal Lane, to be provost. The choice of Lane would have immediate local consequences for Smalley and for nanotechnology research at Rice, but over the longer term it would have even greater consequences for nanotechnology nationally. After seven years as provost, Lane went on to become the director of the National Science Foundation and then presidential science adviser in the 1990s. He was as responsible as any other government official for the founding of the National Nanotechnology Initiative. Through his dealings with Smalley from 1986 to 1993, Lane arrived at a vision for nanoscale science that resembled Smalley's, and he took that vision with him to Washington.

By coincidence, Rice University also installed a new president, George Rupp, in the fall of 1985. By early 1986, Rupp and Neal Lane were looking for initiatives that would enable their administration to put its distinctive stamp on the university. At the same time, a minor but pressing item on their agenda was to keep Rick Smalley from going to a competing university. Eventually they offered a combined solution to both issues. Rupp introduced a new initiative to create multi-disciplinary research centers to "identify niches in which [Rice's] relatively small scale offers us a comparative advantage because collaboration is required from a number of departments—departments that in larger, more differentiated universities are much less likely to interact with each other."[31] The centerpiece of this effort was to be the Rice Quantum Institute, with Smalley as its director.

Rupp's initiative made the RQI considerably more formal and powerful than it had been in the past. For the first time, an executive director—a physicist named Ken Smith—was appointed to run the institute's day-to-day operations. Smith was given a budget to cover part of his salary, a full-time secretary, and "a permanent RQI office complex (two offices and conference room)."[32] Most important, the administration agreed to coordinate with the RQI in adding five new faculty lines in the next five years in the areas of "atomic and molecular theory," "physical chemistry," "electronic materials," "quantum chemistry (theory)," and "microelectronics." Suddenly, Smalley could influence hiring across a wide swath of science and engineering at Rice—and his wish list shows that he used that influence to increase Rice's capacity in fields related to microelectronics.

Smalley and Smith also tried to bring in external funding sufficient to turn the RQI into a "center." By 1986, federal money was again beginning to move more freely into universities, particularly through the vehicle of academic center programs. Thus, Smith energetically cajoled RQI-affiliated faculty members to write a proposal for an NSF Science and Technology Center since "NSF appears to be emphasizing development of collaborative research centers (like RQI!)."[33]

But what kind of center to propose? Smith and King Walters, an influential member of the Rice physics department, summarized three areas in which new hires would "enhance" the institute sufficiently to secure external funding: "atomic and molecular theory," "surface science and the condensed phase," and "microelectronics and the new materials science." In their description of the latter area they "recognized that microelectronics is entering a new era of smaller and vastly more complex devices." "What is less well-known," they continued, "is that both the knowledge of molecular physics and many of its techniques have been (and continue to be) critical in the development of this industry."[34] In other words, the RQI's expertise was already relevant to microelectronics, but Moore's Law would soon magnify that relevance even more. Accordingly, in initial discussions the three leading candidates for an RQI Science and Technology Center proposal were all related to microelectronics: "semiconductor surface chemistry," "magnetic materials" (including magnetic bubble materials—at the time an advanced data-storage medium), and "materials science of electronic materials."[35] Those discussions also emphasized that the RQI would have to develop links to industry:

> We need to develop personal relationships with individuals who have policy-making positions in industry:
>
> electronic materials: IBM (Yorktown), HP, TI, Motorola
> magnetic materials: CDC, 3M, IBM (Phoenix)
> chemical industry: Shell, Exxon, Dow, and other local concerns.[36]

In the end, the RQI faculty opted for a proposal on molecular physics, rather than one of the topics more closely related to microelectronics. That proposal was not awarded funding. Although the RQI continued to influence hiring across the university, the quest to coordinate its faculty around a grand vision for research seemed to have stalled.

"C_{60}, Chapter 2"

Meanwhile, Rick Smalley's personal research interests were drifting away from buckminsterfullerene. By 1990 he was satisfied that he had overcome all possible objections to the C_{60} model that the Rice-Sussex team had proposed. Unfortunately, it wasn't yet possible to make more than minute quantities of C_{60}—not nearly enough to analyze using bulk characterization tools. The amount that could be learned about C_{60} seemed to be disappointingly constrained. As Bob Curl recalled in 2008, so long as production of C_{60} could not be scaled up, there could be no thought of a Nobel Prize for discovering it.[37] Nor was there any chance of commercial application. So Smalley began to wind down his fullerene work and returned to the semiconductor cluster research he had been working on before C_{60}. As the title of a talk by a Smalley student in 1989 put it (perhaps in contrast to C_{60} research), "Silicon Is Never Boring."[38] Then in the early 1990s three discoveries convinced Smalley to return to carbon-cage materials (generically "fullerenes"). First, in 1990 Donald Huffman and Wolfgang Krätschmer discovered an astonishingly simple process for making larger quantities of buckyballs. Simply by running an electric arc across two graphite rods in a helium atmosphere at reduced pressure, they could make enough C_{60} to analyze with an infrared spectrometer. Suddenly, a lot more became known about buckyballs very quickly. In a talk he gave the next year, Smalley referred to this as "C_{60}, Chapter 2."[39]

"Chapter 2," as it turned out, moved quickly away from buckyballs and toward carbon nanotubes. Both were closed cages of pure carbon, but Smalley could foresee much more interesting electrical and mechanical properties for the elongated nanotubes than for their spherical cousins. The year 1991 saw the first production of macroscopic quantities of multiwalled carbon nanotubes. Then in 1993 came the discovery of single-walled nanotubes (or SWNTs) by groups at NEC and IBM. Smalley dubbed these "the world's most perfect material" and dedicated the rest of his career to making, understanding, and applying them.

Because of its difficulties in winning research grants, the RQI was evolving into a primarily pedagogical institution, so Smalley saw a need for a new center to aid his nanotube research. Once again, as in 1985, changes in Rice University's administration came to his aid. In the fall of 1992 George Rupp suddenly resigned as president. A few months later, Bill Clinton was inaugurated as president of the United States and asked Neal Lane to become director of the National Science Foundation. Thus, once again, Rice was to have new people in its top two positions. This time Smalley was put on the search committee for a president rather than that for a provost. Smalley soon applied his forceful personality, often charming though sometimes caustic, to swaying the presidential candidates to his vision for scientific research.

But they probably didn't require much persuading. As in 1986, a new administration would want to start some new initiatives to make its mark. If those initiatives were suggested by a faculty member whom the school was desperate to keep from going to another university, then all the better. When the new president, Malcolm Gillis, arrived, in 1993, he was able to articulate Smalley's vision as his own within weeks. After only four months in office, Gillis laid out the three areas in which he believed Rice's faculty excelled, one of which was the "50 faculty members related to research in nanotechnology—science on the nanometer scale." "And of course," he continued, "we have people like [chemistry Professor Richard] Smalley who are known all over the world."[40] After a year as president, Gillis described his top four achievements; number three was "the emergence of a rapidly emerging program in nanotechnology."[41]

Not coincidentally, number four was that "last year we lost no faculty member we wished to keep. Many other universities came calling, the likes of Princeton and Berkeley, for example, and Stanford and Ohio State and others. But they walked away empty-handed." Princeton and the University of California at Berkeley were two of the schools that had made significant offers to Smalley in that year.[42] Bob Curl suggests that by the early 1990s Smalley was receiving as much in salary and research funding as Rice—or almost any other university—would pay. So when other universities tried to lure him away, he chose to use that leverage over Rice to found a new center instead of asking for additional salary or funding.[43] Smalley also convinced Rice to let him coordinate a cross-departmental research program encompassing, by one estimate, a quarter of Rice's faculty members in science and engineering.[44]

As the quotations above indicate, the focus of that research program was nanotechnology. Over the course of the early 1990s, just as the term "nanotube" was becoming current, Smalley was repeatedly exposed to arguments that "nanotechnology," or science at the nanometer scale, would be the next big research wave. By September of 1992 he had internalized those arguments enough to refer to his own work as "fullerene nanotechnology." By mid 1993 he was being asked to advise Congress about "how [he planned] to build on [his] success with 'buckyballs' to work toward practical, useful nanotechnology."[45]

There has been much interest among historians and sociologists of science in Smalley's nanoscience turn, in view of his later importance in the politics of nanotechnology. Of particular interest has been the fact that Smalley distributed copies of Eric Drexler's book *Engines of Creation: The Coming Era of Nanotechnology* to Rice's administrators and trustees.[46] This has been taken as evidence that, in the early 1990s, Smalley was invested in a flamboyantly futurist vision of nanotechnology—a vision that, in the early 2000s, he would publicly repudiate. As the literary scholar Colin Milburn has commented, "For many years, Smalley fervently supported Drexler's vision."[47]

It is probably true that Eric Drexler helped focus Smalley's attention on nanotechnology, though Smalley's schedule shows that he was exposed to the term at many conferences in 1991 and 1992. By my count he attended at least five conferences with "nano" in their names in those years, and at most of them the title of Smalley's own presentation contained the prefix "nano." Nevertheless, it is true that Smalley distributed copies of Drexler's book, though even as early as January of 1993 he was decidedly ambivalent about Drexler's vision of programmable "assemblers" reanimating corpses atom by atom. As he told Rice's dean of science and dean of engineering,

> Increasingly in the scientific and public press we are seeing the term "nanotechnology." In its most fantastic form, as one reads for example in the popular 1986 book "Engines of Creation: The Coming Era of Nanotechnology" by Eric Drexler, this new field will be the "greatest technological breakthrough in history." Drexler writes of a technology where nanoscopic "assemblers" are controlled by nanocomputers to produce virtually anything one can imagine. … Neat stuff! While these are certainly engaging ideas for science fiction, and perhaps not as foolishly naïve as they may sound, they are not particularly useful for choosing research

> directions over the next decade. At best they are simply too far from current technology to be useful. On the other hand, the central idea of nanotechnology—the ability to arrange atoms into structures engineered on a nanometer scale to serve some useful purpose—is quite reasonable to think about even now.[48]

What Smalley saw in Drexler, then, was not so much the content of his vision as his demonstration that *some* vision for nanotechnology could be appealing and optimistic. From the very beginning, though, the specifics of Smalley's own outlook for nanoscience departed widely from Drexler's and were inspired much more by the hope that molecular electronics—aided by the fullerene research community—would supplant silicon and extend Moore's Law.

To give Rice a point of entry into the evolution of nanotechnology "over the next decade," Smalley proposed that the university build a new Center for Nanoscale Science and Technology (CNST), which he would direct. Ken Smith, executive director of the RQI, would also be executive director of the CNST. Smalley also demanded a new building to house the CNST. President Malcolm Gillis, then moving Rice into a capital campaign, agreed, largely because he estimated that Smalley could be used to elicit as much as $50 million in donations. A headline in the *Houston Chronicle* of November 11, 1993 put it pithily: "New Building to Keep Rice Up with Science and Prof on Campus."[49]

Just as in 1986, too, Smalley negotiated a new round of faculty hiring—a dozen tenure-track positions—over which he would have considerable influence.[50] Several of the hires who would become closely associated with the CNST, including Doug Natelson in Physics and Kevin Kelly in Electrical and Computer Engineering, worked on topics directly related to molecular electronics. That hiring focus was applauded, no doubt, by the members of the CNST advisory board who were also leading participants in DARPA's Moletronics program: Mark Reed, Fraser Stoddart, George Whitesides, and Stan Williams. Once again, molecular electronics—rather than Drexlerian assemblers—was at the heart of Smalley's plans for nanoscience. Certainly, neither Drexler nor any of his associates served on the CNST advisory board.

Meanwhile, as the local nanoscience regime developed at Rice, Smalley and others began to advocate for the adoption of a similar regime at the national level. As Matt Eisler has shown, Smalley's membership on the Department of Energy's Basic Energy Sciences Advisory Committee helped push the DoE toward nanotechnology as a way of organizing its post–Cold War research priorities.[51] Similarly, by December of 1993, only three months after becoming director of the NSF, Neal Lane was already telling journalists that nanotechnology would be the next big thing:

> **Q:** What are some of the new, exciting areas of research that you see on the horizon where NSF will be putting its money? …
> **A:** … There's a whole area talked about of nanotechnology where one hopes to manipulate matter at the atomic, molecular level using light and by other means to construct new kinds of molecules, new kinds of devices. The discovery of buckminsterfullerene, C-60, has exploded into a whole new field of chemistry and materials science. You can make superconductors out of the stuff, you can make semiconductors out of the stuff, you maybe can make microstructures that are strong wires with unusual conducting properties.[52]

At this point, then, Lane was talking about fullerene research as *the* synecdoche for all of nanotechnology—quite naturally, in view of the recent buzz about carbon nanotubes and his personal connection to the birthplace of buckminsterfullerene. Lane's plans for nanoscience would soon expand beyond fullerenes, however—for instance, through the NSF's competition for a National Nanofabrication Users Network the next year. What would *not* change for Lane was the centrality of microelectronics in his vision for nanotechnology. As the quotation above makes clear, for Lane the excitement and purpose of nanoscience, whether embodied by fullerenes or nanofabrication, was conjured not by Drexler's molecular assemblers, but by the keywords of microelectronics: devices, superconductors, semiconductors, wires, conducting properties, and so on.

Center as Networks and as Firms

As the example of the National Nanofabrication Users Network (NNUN) shows, although Rice's Center for Nanoscale Science and Technology was among the first organizations to use the prefix "nano" in its name, it was by no means alone. In the early 1990s, talk about nanotechnology boomed thanks to the combined—and largely uncoordinated—efforts of futurists, science fiction authors, politicians, civil servants, practicing scientists, journalists, and others. As I and others have noted elsewhere, adoption of the label "nano" allowed organizations and fields to signal their contiguity with headlining-earning research breakthroughs, persuasive visions of the future, and emerging trends in the re-organization of science in the United States and elsewhere.[53]

We saw in chapter 5 that the NSF and the leaders of the NNUN at Stanford and especially at Cornell saw the "nano" network as a way to simultaneously tie together microfabrication expertise over a broad disciplinary spectrum spanning the physical, engineering, and life sciences. Richard Smalley's vision was strikingly similar—as seen, for instance, in his frequent description of nanotechnology at Rice as a "symbiosis" between the "distinct" but "interdependent" areas of "wet" nanotechnology ("the study of biological systems which exist primarily in a water environment"), "dry" nanotechnology ("which derives from surface science and physical chemistry [and] focuses on fabrication of structures in carbon (*e.g.* fullerenes and nanotubes), silicon, and other inorganic materials"), and "computational" nanotechnology ("which permits the modeling and simulation of complex nanometer-scale structure").[54]

Remember, too, that the NNUN was designed to coordinate "network" activities across a wide geographical area. That aim, also, was mirrored in Smalley's plans for nanoscience. In the fall of 1998, he submitted a proposal for an NSF Science and Technology Center to be called the Center for Carbon Nanotechnology. As the introduction to the proposal noted, though, the CCN was to be a geographically far-flung enterprise rather than a physically centralized entity:

> Novel aspects of the proposed Center include its breadth, encompassing a relatively large number of senior investigators (17) at eight universities and other participants at four international corporations, three U.S. Government laboratories and one overseas research institute. No individual group will receive vast research funding through the Center and all will find additional sources of support. The Center itself will be just that: a *Center*—it will serve as a locus of thought, communication, and action, within a supportive infrastructure.[55]

An explanatory diagram in the proposal represented the center as four intersecting nanotubes, standing for its four major "Project Areas": Growth Technologies (i.e., synthesizing nanotubes), headed by Smalley; Nanomaterials, headed by John Fischer and Karen Winey at the University of Pennsylvania; Nano-electronics, headed by Mark Reed; and Nano-probes and sensors, headed by Charles Lieber. That is, half of the groups in the CCN would have been headed by veterans of DARPA's Moletronics program. Among the other proposed CCN participants were several other prominent molecular electronics researchers: George Whitesides at Harvard, Jim Heath and Fraser Stoddart at the University of California at Los Angeles, Jim Tour at the University of South Carolina, Phaedon Avouris at IBM, and Cees Dekker at the Delft University of Technology. At the same time, Smalley was urgently trying to recruit several of those people to move to Rice. Rice was in discussions with Reed, Avouris, Lieber, and Jim Gimzewski (a former collaborator with Ari Aviram and Christian Joachim) at various times, and in the fall of 1999 Tour moved to Rice from South Carolina.[56]

Despite a generally favorable site review, the Center for Carbon Nanotechnology was not funded. At Rice, the backroom speculation was that Smalley's ongoing battle with cancer figured in the proposal's failure. The NSF, according to that theory, didn't want to entrust so much of the leadership of the center to a scientist who might have to withdraw for health reasons. That's possible, of course; and it is certainly the case that in Rice's next attempt to obtain NSF funding for a nanotechnology center (detailed below) scientific and administrative leadership fell to younger scientists, even as Smalley continued to guide through vision and charisma.

In the meantime, Smalley embarked on what turned out to be one last venture: a start-up company he co-founded with Ken Smith, Daniel Colbert (another Rice colleague), and Bob Gower (a prominent chemical industry executive). Smalley's research group at Rice had, for some time, been "manufacturing" small quantities of single-walled carbon nanotubes of very high quality and distributing them (for a nominal fee) to researchers around the world. Though the chemical reactor in which the tubes were made was paid for by the Office of Naval Research and housed in a Rice University lab, the scheme for distributing the tubes—named Tubes@Rice—operated like a semi-commercial concern.[57] By 1999, Smalley, Smith, and Colbert were using Tubes@Rice as evidence that a market existed for a company, which they dubbed Carbon Nanotechnologies, Inc. (CNI), that could mass produce nanotubes for a variety of applications.

At the same time, Smalley and Colbert applied pressure on the Rice administration to facilitate patenting by faculty members. In 1998 Rice brought in consultants (including two former presidential science advisers) to help it make the "transition from a 'behind the hedges' ivory-tower institution to one that is outward looking and research oriented" by advising on how to set up a technology transfer office.[58] On the consultants' suggestion, Rice organized a seven-member Patents Committee, to which Colbert was appointed.[59] Thus, Smalley and his allies had considerable influence over the university's policies regarding intellectual property. In fact, a review of the Rice Office of Technology Transfer's activities conducted in 2000 divided its patent applications into three categories: "U.S.," "Foreign," and "Smalley Research." The review found that Smalley's group accounted for seven of the 47 patent applications filed by Rice that year.[60] To help him achieve that pace, Smalley used his influence to get Rice to provide him with help that wasn't available to his colleagues. For instance, Rice engaged Baker and Botts, a prestigious law firm with which the university had had a long relationship, to shepherd the patents CNI would have to license through the long patenting process.

In some ways, CNI was simply the vehicle for fulfilling part of the mission of the canceled Center for Carbon Nanotechnology. As the proposal for the center promised, it would "carry carbon nanotechnology from the laboratory to the marketplace within the Center's life span."[61] That kind of language had been spreading across the US academic research landscape since the late 1960s or even earlier, with quickening pace ever since Erich Bloch's tenure as director of the NSF. What is notable with respect to the present narrative, however, is the extent to which molecular electronics and the long arm of Moore's Law influenced Smalley's vision for academic entrepreneurship. For instance, in 1998, just as he was preparing to found CNI, Smalley answered *Business Week's* questions about nanotubes almost exclusively in terms of their application to molecular electronics and to the extension of Moore's Law:

> **Q:** What makes buckytubes compelling to industry?
>
> **A:** Take a look at the preface and introductory sections of SEMATECH's *National Technology Road Map for the Semiconductor Industry*, 1997. … The notion that they will have to leave silicon was discussed in depth. They see so many problems on the horizon that they can't get around. So now they are ready to think about things like carbon.
>
> **Q:** That's a big departure.
>
> **A:** Yes. And this gets back to the old dreams of "molecular electronics."… There is a huge electronics industry, well in excess of $200 billion a year, with a great desire to maintain Moore's Law for another 50 years. It's likely that tens of billions of dollars will be spent on breaking the 100-nanometer barrier. And the only thing on the other side of that barrier is molecular electronics. … In the 1970s, there was much discussion of molecular electronics, but nothing came of it, mostly because people didn't have good molecular metallic wires. But now it looks like we do, and the name is "buckytube."[62]

That is, Smalley believed that CNI could make a profit by supplying carbon nanotubes to IBM (which was then making headlines for using nanotubes in a new kind of experimental transistor) and to other microelectronics companies.[63]

CNI's nanotubes never made their way into IBM's commercial transistors, or anyone's, in large part for the same reason that Josephson computing and the earlier generations of molecular electronics didn't succeed. Silicon moved right past all its competitors, as predicted by Moore's Law. Nevertheless, CNI did make inroads into the electronics industry. The Korean electronics company Samsung, for instance, repeatedly came close to marketing a display system incorporating nanotube emitters (some bought from CNI).[64] Other manufacturers of mobile phones, too, experimented with using nanotubes as additives in their glass touch screens.[65] In 2007, CNI merged with another firm (Unidym), which was then sold in 2011 to another Korean electronics company (Wisepower), "a leading supplier of Li-polymer batteries for mobile appliances."[66] Today, Unidym's corporate slogan is "carbon for electronics." Its biggest success seems to have come from selling nanotubes to Entegris, a maker of the trays on which silicon wafers are carried in semiconductor fabs—an application where even the tiny decrease in dust and flaking caused by incorporating nanotubes into the tray's plastic matrix could justify the enormous expense of using one of the world's most exotic materials.[67] The demands imposed by semiconductor manufacturing are so stringent, and propagate through so many technologies, that when a company such as CNI finds that it can't attach itself to Moore's Law in one way (e.g., by producing a material used in commercial transistors), it can almost always find some other way (e.g., by producing a material used in the *making* of commercial transistors).

Life after Moletronics

Thus, for Richard Smalley—one of the world's leading advocates for organizing science around the nanoscale—nanotechnology was largely about microelectronics. Smalley also spoke of nanotechnology's potential applications in biomedicine and in space travel (including a space "elevator"), and in his final years he increasingly spoke of its potential usefulness in producing and transmitting energy. But the one continuous thread in his nanoscience work—from semiconductor clusters to carbon nanotubes to recruiting molecular electronics researchers to advise or join Rice's nanoscience efforts—was microelectronics.

Smalley wasn't alone in thinking that electronics (along with medicine) would be one of the most commercially and intellectually fruitful applications of fullerenes and of nanotechnology more generally. By the late 1990s, Smalley and various veterans of the DARPA Moletronics program were almost mandatory participants in the conferences and congressional panels that led to the establishment of the National Nanotechnology Initiative, presumably because policy makers believed that fullerenes and/as molecular electronics provided the most plausible yet compelling examples of what nanoscience was and what it would become. If nanotechnology had an emblematic material in the late 1990s, it was the fullerene, and if nanoscience had an emblematic world-changing application that policy makers invoked, it was as a replacement for silicon via breakthroughs in molecular electronics.

In the early 2000s, therefore, Smalley appeared alongside Stan Williams, Jim Heath, Mark Ratner, Charlie Lieber, George Whitesides, James Ellenbogen, Phil Kuekes, Paul McEuen, Phaedon Avouris, Cees Dekker, and other leading molecular electronics researchers at events intended to articulate a compelling vision for nanotechnology, and especially for federal funding of R&D in nanotechnology. Notably, this crew appeared regularly at meetings that the National Nanotechnology Initiative held to generate enthusiasm for nanotechnology among politicians and other federal and state agencies. In the spring and summer of 2002 alone, Smalley was a featured speaker at conferences sponsored by the National Science Foundation, the Department of State, NASA, and the Department of Commerce, sharing the stage with at least one notable figure from molecular electronics at each.[68]

Unfortunately, the lavish attention paid to molecular electronics was premised on—and demanded—an unsustainable degree of optimism about the near-term overthrow of silicon. I say “unsustainable” because between 2000 and 2002 molecular electronics was the vehicle for an unprecedented case of scientific fraud that permanently tarnished the field’s reputation. In those years, a researcher at Bell Labs, Jan-Hendrik Schön, published numerous articles in high-profile journals claiming to have achieved most of the goals that molecular electronics researchers had been aiming for ever since the 1974 Aviram-Ratner paper—including a single-molecule transistor. Nor did Schön limit himself to any single approach to beyond-silicon electronics—he also published claims of groundbreaking achievements in novel Josephson junctions and superconducting fullerenes. If his results had been real, Schön would have been one of the few individuals to personally connect all the different exotic electronics fields surveyed in this book. In fact, it was all a fiction; Schön was making up data wholesale. Instead of doing experiments, he was manufacturing findings that matched what other scientists had predicted would someday be achievable.

Schön was a uniquely bad actor. Forrest Carter and some other molecular electronics researchers may have made wild predictions, but they never claimed that those predictions had already come true. The Yale–Rice–Penn State and HP-UCLA teams may have interpreted their experiments generously, but they had actually performed those experiments. Rightly or wrongly, though, Schön’s fraud cast a long shadow over both experiments in and predictions about molecular electronics.

The philosopher Alfred Nordmann speaks for many of the field's critics when he writes that Schön was enabled by the pervasive "ontological indifference" of nanotechnologists in general and of molecular electronics researchers in particular.[69] Nanotechnology, Nordmann claims, exemplifies the drift away from the pursuit of fundamental discoveries and toward the valorization of new capabilities or objects. Schön simply over-internalized that indifference and came to care so little about knowledge claims (and assumed his audience would similarly not care) that he saw nothing wrong with a little doctoring of data.

Nordmann has a point, though it is important to emphasize that many molecular electronics researchers were decidedly not indifferent to the ontological status of Schön's circuits (or anyone else's). After all, some of the participants in the DARPA Moletronics program, particularly Paul McEuen, were instrumental in exposing Schön precisely because they displayed the scientific skepticism that Nordmann says is disappearing. And, as Eugenie Samuel Reich has shown, Schön's fraud was *not* a victimless crime: some molecular electronics researchers wasted time and resources trying to replicate his results.[70] Indeed, a Rice University physicist, Doug Natelson, figures prominently in Reich's examination of the Schön case for his futile attempts to apply the old-fashioned test of replication to Schön's "findings."

Nordmann is surely right, though, that some of Natelson's colleagues were incautiously credulous about Schön's results, in a way that the field's critics could easily interpret as indifference. For instance, a *New York Times* article on Schön before his downfall offered this assessment of his work:

> "It is really, really nice work that will influence the field a lot," said Dr. James Tour, a professor of chemistry at Rice University. "They [Schön and his collaborators] hit on something really big."[71]

In the aftermath of Schön's defrocking, *The Guardian* quoted James Heath of UCLA as having said this:

> "I saw these [Schön's] results being presented to a German audience" … and they knock on the chairs instead of clapping. It was incredible—they got a 'standing knocking.' I thought, these guys are going to Stockholm."[72]

And a syndicated Associated Press article contained this passage:

> "The science looked to be absolutely beautiful," said Charles Lieber, a professor of chemistry at Harvard University. … What he claimed to have done was not theoretically impossible, just supremely difficult. It was something experimental physicists often attribute to "magic hands," an ineffable gift for the mechanics of their field. "It made sense," Lieber said.[73]

Schön's fraud did not, of course, destroy the field of molecular electronics. Scientific journals and the popular press continued to publish accounts of new, faster devices based on ever more exotic molecular configurations. But molecular electronics was different after the Schön affair in two important ways. First, the field once again shifted its focus to a new class of materials. Recall that the Aviram-Ratner-Carter generation of molecular electronics had been based largely on variants of charge-transfer salts and conducting polymers. In the 1990s, the field was strongly oriented toward C_{60}, carbon nanotubes, and supramolecular conformational switches such as rotaxanes. Since 2004, though, hopes for commercial progress in molecular electronics have largely been pinned on graphene, a material made up of single planar sheets of carbon atoms. Graphene does, indeed, have unusual properties that justify some optimism about its potential. In particular, it is a "semi-metal"—a semiconductor with only a negligible energy gap between the empty conduction band, in which electrons can move, and the valence band "filled" with relatively immobile electrons.

Yet the crush of interest in graphene is so reminiscent of earlier failed bandwagons in electronic materials that it has prompted both enthusiasm and anxiety. For instance, as Doug Natelson observes,

> The graphene craze continues unabated. Remember how the superconductivity community descended upon MgB2 and made every superconductivity-related measurement under the sun on the new material in a feeding frenzy? A similar phenomena is taking place with the 2d electron community and graphene. Fortunately, graphene seems to be pretty neat stuff![74]
>
> The 2010 Nobel Prize in Physics has been awarded to Andre Geim and Konstantin Novoselov for graphene. Congratulations to them! Graphene, the single-atomic-layer limit of graphite, has been a very hot topic in condensed matter physics since late 2004. … There is no question that graphene is a very interesting material, and the possibility of serious technological applications looms large, but as Joerg Haber points out, overhype is a real danger. The prize is somewhat unusual in that it was very fast on the scale of these things.[75]

Or, more succinctly, as the blog post referenced by Natelson put it,

> Great, the physics Nobel Prize for graphene! Now don't overhype it.[76]

The second way in which molecular electronics changed after the Schön affair is that the "DARPA generation" became more circumspect and somewhat less visible. The *New York Times*, for instance, routinely quoted Reed, Tour, Heath, Williams, et al. up to early 2003, but since then only Williams has appeared much in the *Times* (and even then primarily in connection with non-"molecular" electronics and optoelectronics).

Notably, several prominent members of the “DARPA generation” moved away from molecular electronics and toward the life sciences. Some simply added topics such as “applications for nanoelectronics in chemistry and biology” or “nano-bio interface” to a research portfolio oriented primarily toward nanoelectronics for information processing; others, though, made truly significant moves away from molecular electronics and toward topics such as neuroscience.[77] In particular, after graphene burst onto the scene, Jim Tour refocused his group primarily on “nano-cars” (nanotube-based “vehicles,” primarily for drug delivery). Even before graphene, Jim Heath had refocused his group on cancer (and other disease) therapies.

The move from electronics to the life sciences was, in some ways, a reversal of the way the field operated before Schön. We saw in chapter 3 that one of the distinguishing features of molecular electronics ever since the 1974 Aviram-Ratner paper has been the use of biology as an existence proof for “molecular” signal transmission. In the Forrest Carter era in the 1980s, researchers at the margins of the biotechnology industry and the life sciences (e.g., rhodopsinists) embraced molecular electronics and suggested that biological systems offered concrete models for molecular computing architectures and that organisms or their components could be used to fabricate features in integrated circuits. In the 1990s, “nanotechnology” became a powerful organizing principle for binding “wet” and “dry” approaches together in molecular electronics and other fields.

After the Schön affair and the other controversies that constituted molecular electronics’ “midlife crisis” in 2002–2003, however, some researchers increasingly used the electronics–life science nexus to try to escape from the quagmire of electronics and get into funding-rich and more obviously altruistic topics in biomedicine: cancer cures, fighting bioterrorism, regenerating neurons, and so on.[78] Not all researchers took that path, of course, but many did. In fact, that shift at the individual level was accompanied by similar transitions within organizations such as the Cornell Nanofabrication Facility. Once “nano-” gets into the prefix of an organization, it provides an avenue for escape to the life sciences whenever an emphasis on the physical or engineering sciences becomes untenable or undesirable.

Again, we can return to the example of Rice University and Rick Smalley to see how this worked. After the failure of the proposal for a Center for Carbon Nanotechnology under the NSF’s Science and Technology Centers program, Smalley threw himself into advocacy for nanotechnology and into his new start-up company, while continuing to lead an on-campus fullerene research group. In 2000, though, the NSF announced a new centers program, the Nanoscale Science and Engineering Centers, as the primary funding instrument through which the NSF would participate in the nascent National Nanotechnology Initiative. Once more, Smalley pushed for Rice to pursue an NSF-funded center, but this time leadership of the proposal fell to two younger faculty members: Vicki Colvin in Chemistry and Mark Wiesner in Civil and Environmental Engineering.

The proposal that emerged and was successfully funded, therefore, was for a Center for Biological and Environmental Nanotechnology (CBEN), not a center in molecular electronics or carbon nanotechnology. Both those areas were still represented in the CBEN, just not as its central theme. As a presentation to the NSF in 2001 put it, the "motivation" of the center would be as follows:

> - Nano in Bio → Medicine …
> - Bio in Nano → Materials/Devices
>
> Manipulation and assembly of nanostructures from the dry side into materials and functional devices using machinery from the wet side will enable new technologies
>
> - Nano-Bio in our Environment → Responsibility
>
> Risk Assessment/Risk Reduction[79]

Here we see the vestiges of molecular electronics in the "Bio in Nano" research area, and fullerenes were employed in all three areas; however, the organizing principle of the center as a whole was biological, not electronic. And since most of the researchers affiliated with the center were housed in engineering or physical science departments, rather than in the life sciences, the center operated very much as a nanoscience-enabled pathway for physical and engineering scientists to attach to the life sciences, rather than vice versa.

We probably should understand his much-publicized dispute with Eric Drexler in the pages of *Scientific American* and *Chemical & Engineering News* from 2001 to 2003 in the context of Smalley's involvement with the CBEN.[80] Again, much has been said about this "Drexler-Smalley Debate" in the literature on social studies of nanotechnology.[81] Smalley's apparent animosity toward Drexler, with the backing of leaders of the National Nanotechnology Initiative and at the invitation of the editors of two prominent journals, makes for compelling fare. As I noted earlier in this chapter, Smalley's skepticism about Drexler's technical competence had a long history (his qualms about assemblers were relatively consistent from the beginning), but his willingness to wage a public *ad hominem* battle with Drexler was something quite new.

What earlier studies of the Drexler-Smalley debate have missed, though, is the role of Smalley's local ambitions and organizational requirements in shaping his half of the contest. Both Smalley and Drexler have been criticized for indulging in hyperbolic fearmongering in order to discredit each other, most egregiously with Smalley's admonition to Drexler that "you and people around you have scared our children." Yet Smalley's mention of children probably was connected to preoccupations arising from programs that had been proposed in the CCN and in the CBEN. Both centers proposed a number of mechanisms whereby on-campus research could "translate" to the marketplace. But both centers also included mechanisms whereby academic scientists and engineers could interact with a wide variety of stakeholders—from industry, of course, but also from non-governmental organizations, museums, civic groups, and, above all, elementary and secondary schools, especially those serving populations underrepresented in the US science and engineering workforce.[82] Thus, in the context of building centers that would connect academic nanotechnologists with the wider world, children were more than figments of Smalley's hyperbolic rhetoric—they were the key to securing resources and political goodwill.

The CBEN, in particular, also included a number of mechanisms to ensure "responsible innovation," whereby the means and ends of academic research would maintain contact with the desires of civil society. This aspect of nanotechnology—at Rice University and in general—has attracted much interest in the social studies of nanotechnology.[83] Indeed, Smalley and the CBEN tie together most of the themes that preoccupy scholars working on what the National Nanotechnology Initiative labels "ethical, legal, and social implications" (ELSI) of nanotechnology: the role of futuristic visions and debates about where the science/fiction line might lie, the toxicological and environmental consequences of nanomaterials (especially nanotubes), and the gradual process of bringing new stakeholders (including humanists and social scientists) into the crafting of technoscientific knowledge.[84] These are all important topics, of course. But in this chapter and elsewhere in this book I have tried to show that each of these facets of the nanotechnology enterprise has been continuously and deeply shaped by the evolving organization and technical capacity of the microelectronics industry—a fact not much acknowledged in the social studies of nanotechnology.

Responsible innovation is, in a complicated way, an outgrowth of the idealistic upheavals of the Vietnam era covered in chapter 1. So is the interdisciplinarity that defines nanotechnology, including expansive notions of interdisciplinarity in which natural scientists, engineering scientists, social scientists, and humanists commingle. The futurism that nanotechnologists continually invoke *and* reject is nothing new; we saw in chapter 3 that this has been an irrepressible feature of molecular electronics since the 1970s, if not since the 1950s, and that molecular electronics was at the heart of Rick Smalley's nano-futurism. And the flight to the life sciences that the CBEN typified had similarly complex motivations that—as we saw in chapters 1 and 5—were impelled by shifts in the microelectronics industry's R&D capacity and in the geopolitical importance attached to the physical sciences. Nanotechnology is no mere appendage to microelectronics, of course. But the politics, the ethics, and the economics of nanotechnology aren't intelligible unless one reckons with the ubiquitous, multivalent presence of Moore's Law in the decisions made by nanotechnologists and their interlocutors.

Epilogue

This book's narrative has traveled from the late 1950s to the early 2010s, with particular emphasis on the first thirty years of Moore's Law (1965–1995). Through a series of somewhat interconnected case studies, I have tried to show that during that period the microelectronics industry gave impetus to many large-scale changes in the conduct, the organization, the aims, and the tools of American science. Leaders of microelectronics firms intended to provoke some of those changes; others arose accidentally from the continual restructuring and globalization of a colossal and influential industry. In this epilogue, I will distill a few of the most important changes in American science between the era of student unrest in the late 1960s and the dot-com boom/bust, terror attacks, and wars of the early twenty-first century. The changes in American science that I enumerate were by no means due solely to the multi-faceted, propagating influence of the microelectronics industry. Yet any attempt to understand these changes that fails to acknowledge the role of that industry will be lacking in explanatory purchase.

Two big changes took place in the late 1960s and the early 1970s that cast a shadow over everything in this book and over late–Cold War and post–Cold War American science more generally. First, American science civilianized. Not completely, of course, and not as much as many hoped. In the federal budget, military and civilian R&D funding converged dramatically in the 1970s and the 1990s, though the gap grew again in the 1980s and the 2000s. Nevertheless, the general trend since the late 1960s has been for high-tech firms' technology development and scientific research in firms, universities, and government agencies to become less dependent on national-security aims and on support from intelligence agencies and the military services. Second, American science economized—both in the sense that all funders of research (military, civilian, corporate, and philanthropic) wanted greater and more accountable return on their investment and in the sense that economic justifications for research became more influential and more prevalent.[1]

These two fundamental changes are not entirely separable. As I have repeatedly emphasized, many of the institutional experiments in American science in the 1970s and the 1980s were motivated by the increasing market share, across many industries, of Asian and Western European firms. These firms, especially those in Japan, were said to pose a severe threat to the US economy *and* to US national security. The aid given to these firms by their governments was taken as a pretext for the federal government (and many state governments) to intervene, and in particular to adapt the (purported) Japanese model of state-coordinated networks to an American context.

One thread tying these two changes together was the US semiconductor industry. Many industries in the United States saw their market shares plummet in the 1970s, but few provoked federal action the way global competition in semiconductor manufacturing did. That was partly a consequence of the view that microelectronics was a clean, post-industrial, high-tech kind of manufacturing that, like biotechnology, would lead the American economy into the future in a way that, say, the steel industry or the textile industry would not. Perhaps more important, by 1975 both the US military and the US civilian economy had become so suffused with advanced integrated circuits that military and civilian agencies could eagerly collaborate to aid the domestic semiconductor industry. Few other industries elided the distinction between national security and economic security so completely. The energy industry and the aerospace industry are the most comparable cases, and those industries have also had long arms that reshaped American science.

From these two big changes—civilianization and economization—flowed a wide variety of sometimes unexpected consequences. In the late 1960s, the national desire for a more civilian and more accountable research enterprise led to a variety of federal experiments (such as the NSF's Research Applied to National Needs program) and to a variety of grassroots shifts toward civilian, interdisciplinary, applied research (such as those that occurred at Stanford University). The era's budgetary strains, however, meant that experiments such as the Research Applied to National Needs program came at the expense of the generous federal funding for academic buildings and equipment that typified the early Cold War (especially in the immediate post-Sputnik period). By the mid 1970s, many experiments in civilianization were running into insurmountable opposition from politicians and from the scientific community. In response, federal funders—both military and civilian—sought ways to replenish the depleted infrastructure of research, but to do so in the most cost-effective manner possible. Center programs and shared user facilities therefore gained currency as effective approaches.

At the same time, strained industrial budgets and greater demands for accountable returns on corporate investment in research led high-tech manufacturers to cut back on their in-house capacity for basic research and to outsource such research to a variety of jointly funded entities. Industrial research consortia came into vogue, especially in the semiconductor industry. But so did academic centers and facilities, which could be funded by similar consortia of firms and state and federal agencies. Academic centers have a long history, of course, but in the late 1970s they began to proliferate, often mirroring or partnering with the burgeoning industrial research consortia. Again, research areas connected to microelectronics were at the forefront.

Academic center programs gained further popularity in the late 1980s because they could be made to serve an astonishing variety of purposes. They satisfied some of these ends simultaneously, others serially as one purpose receded and another took its place. The versatility of the center form meant it could be endlessly adapted as the political, economic, cultural, and scientific environment changed. Some centers offered access to increasingly expensive research equipment. Others made practitioners from different research areas visible to one another, and provided support for their collaborations, as happened at the University of Pennsylvania in the early days of organic conductors research. Others mediated between universities and industry—though, as we saw at Stanford and Cornell, sometimes organizations set up to benefit one industry (e.g., microelectronics) yielded academic partnerships with another (e.g., biotechnology).

Other national priorities that were implemented through centers and facilities included providing ways for underrepresented groups to participate more—qualitatively and quantitatively—in the US research enterprise. That was one aim of Howard University's membership in the National Nanofabrication Users Network's and of "outreach" programs to predominantly Hispanic and African-American high schools and universities through Rice University's (proposed) Center for Carbon Nanotechnology and its (actual) Center for Biological and Environmental Nanotechnology.[2] Above all, of course, academic centers and facilities were mandated to train personnel—often through interdisciplinary or applied degree programs that could not be housed in traditional departments, such as the Rice Quantum Institute's applied physics PhD or the nanotechnology PhD certificate administered by the University of Washington's Center for Nanotechnology.

By the early 1990s, almost any national or scientific problem could be used as justification to create or modify an academic center, and almost every academic center was being required to address multiple national crises at once. As centers, facilities, and consortia proliferated, their sponsors began to develop ways of funding, and occasionally coordinating, multiple centers all at once. The most influential model for these center programs was ARPA's Materials Research Laboratories program, founded in the early 1960s. Many of the MRL sites served as kernels around which later centers were grown. Still, the MRLs weren't the only model. The National Laboratories, the global archipelagoes of corporate labs established by companies such as IBM, and the National Science Foundation's Regional Instrumentation Facilities all reinforced the idea that center *programs* could do some things more effectively than individual centers and facilities could. Centers and facilities linked to the microelectronics industry served as important models for the growth of the concept of center programs, and centers dedicated to semiconductor and microelectronics topics were usually among the first to be proposed and to gain funding in each successive wave of center programs.

As the US research ecology came to be populated by an ever-increasing array of centers, facilities, and consortia, those organizations began to link up, forming a complex topology of networks of networks. On the industrial side, microelectronics manufacturing presented an early and particularly dizzying example of a network of networks. In the 1970s and the 1980s, Intel and other firms farmed many of the functions of a vertically integrated company out to networks of competing but co-dependent suppliers. As semiconductor firms farmed out much of their in-house research (especially basic or long-range research), scientists and engineering scientists began to flow out of semiconductor firms and into universities, trade associations, consortia, and government agencies. They brought with them industrial connections and patrons as well as a nuanced understanding of how to bounce between fundamental, curiosity-driven research and commercial, technological development—what I have called the zig-zag model of innovation.

They then applied that model in constructing new networks that permeated an ever-expanding range of academic disciplines: organic chemistry, biomedicine and the life sciences, the earth, planetary, and environmental sciences, and eventually the social sciences and the humanities. Some former corporate microelectronics researchers formed interdisciplinary collaborations out of a sense that the barriers to their research affecting their old industry had become so high that they would have to seek other pursuits. Some were presumably so inspired by the success of academic entrepreneurship in biotechnology (and, later, in the dot-com industry) that they formed or allied with firms in those sectors. Others may have seen in the new, networked interdisciplinarity a source of new funding, or opportunities for education "reform," or platforms for more "responsible" and participatory innovation, or simply a chance to refresh modes of research that had become stale.

The multifarious ways in which the semiconductor industry influenced the evolution of American science in the late twentieth century mean that Moore's Law has become more influential than ever and yet harder to locate than ever. It reaches into everything, yet its reach becomes ever easier to miss. The semiconductor industry has contributed greatly to the hybridity of many US research organizations, whereby university and government labs operate quasi-commercially and firms operate quasi-academically or quasi-philanthropically; yet that hybridity makes it harder and harder to say which organizations are in fact pushing Moore's Law forward. That same hybridity suffuses individual career paths and the research trajectories of entire fields, as almost anyone and almost any discipline can claim relevance to the extension of miniaturization—but, conversely, expertise in miniaturization increasingly serves as a means to flee from semiconductor research and into curing cancer, remediating pollution, and mapping the brain.

The ubiquitous invisibility of Moore's Law greatly complicates any effort to tinker with the governance of science and innovation. Just as with climate change, alternative energy, and other areas in which science might provide some useful answers, the governance reforms needed to unearth and/or enact those answers are stymied by the multiple ways in which stakeholders participate in the governance of science. We can all speak admiringly of responsible innovation; however, so long as we demand faster computers, phones, and gadgets, we are all responsible for the war-fueling plunder of the minerals needed to keep Moore's Law going and for the ubiquitous surveillance of each by each that Moore's Law affords. We can debate the propriety of academic entrepreneurship or of government participation in market building; we can lament the mercenary commercial valorization of applied research or the elitist ivory-tower valorization of basic research; we can applaud or condemn the undermining of the traditional academic disciplines; we can quarrel over the right balance of military and civilian agencies' support for research—but any proposals for changing these aspects of the research enterprise mean little until we recognize that they have been co-produced with contemporary society's dependence on ubiquitous, high-speed, digital computation, communication, information, and entertainment.

Notes

Introduction

1. According to the US government's Bureau of Economic Analysis, the "computer and electronic products" industry is consistently the manufacturing industry that contributes most to US GDP in the durable goods category. Overall in the manufacturing sector, computer and electronic products is roughly tied for top place with (or slightly ahead of) chemical products (part of the nondurable goods category). Source: Bureau of Economic Analysis, "Value Added by Industry as a Percentage of Gross Domestic Product," at http://www.bea.gov/iTable/iTable.cfm?ReqID=5&step=1&cid.

2. Alfred Chandler (with Takashi Hikino and Andrew von Nordenflycht), *Inventing the Electronic Century: The Epic Story of the Consumer Electronics and Computer Industries* (Harvard University Press, 2005).

3. On "re-globalization," see David Edgerton, *The Shock of the Old: Technology and Global History since 1900* (Oxford University Press, 2007). On electronics manufacturing and economic growth in Asia, see Dieter Ernst, Tom Ganiatsos, and Lynn Mytelka, eds., *Technological Capabilities and Export Success in Asia* (Routledge, 1998); Larry E. Westphal, "Technology Strategies for Economic Development in a Fast Changing Global Economy," *Economics of Innovation and New Technology* 11.4–5 (2002): 275–320.

4. Paul E. Ceruzzi, *A History of Modern Computing* (MIT Press, 2003).

5. For hands-on know-how, see Christophe Lécuyer and David C. Brock, "The Materiality of Microelectronics," *History & Technology* 22.3 (2006): 301–325. For an argument that transistor development was accelerated by William Shockley's (and to some extent John Bardeen's) hunger to rack up theory-driven discoveries, see Joel N. Shurkin, *Broken Genius: The Rise and Fall of William Shockley, Creator of the Electronic Age* (Macmillan, 2006).

6. Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America* (MIT Press, 1996); Atsushi Akera, *Calculating a Natural World: Scientists, Engineers, and Computers during the Rise of US Cold War Research* (MIT Press, 2007).

7. See, for instance, Jon R. Lindsay, "'War upon the Map': User Innovation in American Military Software," *Technology and Culture* 51.3 (2010): 619–651; Timothy Lenoir and Henry Lowood, "Theaters of War: The Military-Entertainment Complex," in *Collection, Laboratory, Theater: Scenes of Knowledge in the 17th Century*, ed. Helmar Schramm, Ludger Schwarte, and Jan Lazardig (de Gruyter, 2005).

8. Thierry Bardini, *Bootstrapping: Douglas Engelbart, Coevolution, and the Origins of Personal Computing* (Stanford University Press, 2000).

9. Christophe Lécuyer, *Making Silicon Valley: Innovation and the Growth of High Tech, 1930–1970* (MIT Press, 2006).

10. Richard N. Langlois and W. Edward Steinmuller, "The Evolution of Competitive Advantage in the Worldwide Semiconductor Industry, 1947–1996," in *Sources of Industrial Leadership: Studies of Seven Industries*, ed. David C. Mowery and Richard R. Nelson (Cambridge University Press, 1999).

11. For some contemporary editorials in *Science* (the US scientific community's newspaper of record) describing these changes, see Edward E. David Jr., "The Industrial Connection," *Science* 203 (March 2, 1979): 837–840; Dennis J. Prager and Gilbert S. Omenn, "Research, Innovation, and University-Industry Linkages," *Science* 207 (January 25, 1980): 379–384; Nicholas Wade, "Gene Goldrush Splits Harvard, Worries Brokers," *Science* 210 (November 21, 1980): 878–879; Dorothy Nelkin, "Intellectual Property: The Control of Scientific Information," *Science* 216 (May 14, 1982): 704–708; John Walsh, "Bell Labs on the Brink," *Science* 221 (September 23, 1983): 1267–1269; Mark Crawford, "Broader R&D Role Sought for Commerce," *Science* 237 (July 3, 1987): 19; Radford Byerly Jr. and Roger A. Pielke Jr., "The Changing Ecology of United States Science," *Science* 269 (September 15, 1995): 1531–1532.

12. Paul Forman, "The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology," *History and Technology* 23 (2007): 1–152; Alfred Nordmann, Hans Radder, and Gregor Schiemann, eds., *Science Transformed? Debating Claims of an Epochal Break* (University of Pittsburgh Press, 2011). The "epochal break" notion is roughly consonant with the "Mode 1/Mode 2" concept, which claims that the scientific enterprise ran in a basic research-driven mode until the early 1980s; see Michael Gibbons et al., *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies* (SAGE, 1994). A similar putative break and transition to a more profit-driven mode was also (quite influentially) claimed in John Ziman, *Real Science: What It Is and What It Means* (Cambridge University Press, 2000).

13. For a before-and-after view of how the decline in corporate basic research affected the greatest industrial research center of them all, Bell Laboratories, see Jon Gertner, *The Idea Factory: Bell Labs and the Great Age of American Innovation* (Penguin, 2012) and Eugenie Samuel Reich, *Plastic Fantastic: How the Biggest Fraud in Physics Shook the Scientific World* (Palgrave Macmillan, 2009). Reich's book is also something

of an exposé of how changes in scientific publishing in the last few decades have, in all likelihood, made scientific fraud more systemic and easier to get away with.

14. Juan C. Lucena, *Defending the Nation: US Policymaking to Create Scientists and Engineers from Sputnik to the 'War against Terrorism'* (University Press of America, 2005); Roger L. Geiger, *Research and Relevant Knowledge: American Research Universities since World War II* (Oxford University Press, 1993).

15. For a critical perspective, see Derek Bok, *Universities in the Marketplace: The Commercialization of Higher Education* (Princeton University Press, 2003). For a more positive view of universities' participation in markets, see Henry Etzkowitz, Andrew Webster, and Peter Healey, eds., *Capitalizing Knowledge: New Intersections of Industry and Academia* (SUNY Press, 1998).

16. For contemporary observations of some of these changes, see Jerome B. Wiesner, "Universities and the Federal Government: A Troubled Relationship," *Chemical and Engineering News* 56.50 (December 1, 1978): 31–36; Laurence Berlowitz et al., "Instrumentation Needs of Research Universities," *Science* 211 (March 6, 1981): 1013–1018; *An Assessment of the Needs for Equipment, Instrumentation, and Facilities for University Research in Science and Engineering* (National Academy of Sciences, 1971); Philip H. Abelson, "International Competition in High Technology," *Science* 219 (March 4, 1983): 1025; Joseph Palca, "NSF Centers Rise above the Storm," *Science* 251 (January 4, 1991): 19–22.

17. Philip Mirowski, *Science-Mart: Privatizing American Science* (Harvard University Press, 2011).

18. David C. Mowery and Nathan Rosenberg, *Technology and the Pursuit of Economic Growth* (Cambridge University Press, 1989).

19. David C. Mowery, Richard R. Nelson, Bhaven N. Sampat, and Arvids A. Ziedonis, *Ivory Tower and Industrial Innovation: University-Industry Technology Transfer before and after the Bayh-Dole Act in the United States* (Stanford Business Books, 2004).

20. See Christophe Lécuyer, "Semiconductor Innovation and Entrepreneurship at Three University of California Campuses" and Martin Kenney, David C. Mowery, and Donald Patton, "Electrical Engineering and Computer Science at UC Berkeley and in the Silicon Valley: Modes of Regional Engagement," both in *Public Universities and Regional Growth: Insights from the University of California*, ed. Martin Kenney and David C. Mowery (Stanford University Press, 2014).

21. Cyrus C. M. Mody and Andrew J. Nelson, "'A Towering Virtue of Necessity': Computer Music at Vietnam-Era Stanford," *Osiris* 28.1 (2013): 254–277; Cyrus C. M. Mody, "University in a Garage: Instrumentation and Innovation from UC Santa Barbara," in *Public Universities and Regional Growth: Insights from the University of California*, ed. Martin Kenney and David C. Mowery (Stanford University Press, 2014);

Eric J. Vettel, *Biotech: The Countercultural Origins of an Industry* (University of Pennsylvania Press, 2005).

22. Elizabeth Popp Berman, *Creating the Market University: How Academic Science Became an Economic Engine* (Princeton University Press, 2012); Jeannette Colyvas, From Divergent Meanings to Common Practices: Institutionalization Processes and the Commercialization of University Research, PhD dissertation, Stanford University, 2007; Peter J. Westwick, *Into the Black: JPL and the American Space Program, 1976–2004* (Yale University Press, 2007); Daniel J. Kevles, "Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project," *Historical Studies in the Physical and Biological Sciences* 27 (1997): 269–297; Peter Galison, "The Pyramid and the Ring," George B. Pegram Lecture, Brookhaven National Laboratory, Upton, NY, November 3, 2006 (http://www.osti.gov/scitech/?purl=%252F1007947-JyPvKD%252F).

23. W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and a Limitless Future* (Princeton University Press, 2013).

24. Mark Peter Jones, Biotech's Perfect Climate: The Hybritech Story, PhD dissertation, University of California, San Diego, 2005; Sally Smith Hughes, *Genentech: The Beginnings of Biotech* (University of Chicago Press, 2011); Doogab Yi, The Recombinant University: Genetic Engineering and the Emergence of Biotechnology at Stanford, 1959–1980, PhD dissertation, Princeton University, 2008; Myles W. Jackson, *The Genealogy of a Gene: Patents, HIV/AIDS, and Race* (MIT Press, 2015); Walter W. Powell and Jason Owen-Smith, "Knowledge Networks as Channels and Conduits: The Effects of Spillovers in the Boston Biotechnology Community," *Organization Science* 15 (2004): 5–21; Walter W. Powell, Douglas R. White, Kenneth W. Koput, and Jason Owen-Smith, "Network Dynamics and Field Evolution: The Growth of Interorganizational Collaboration in the Life Sciences," *American Journal of Sociology* 110 (2005): 1132–1205; Fiona Murray, "The Role of Academic Inventors in Entrepreneurial Firms: Sharing the Laboratory Life," *Research Policy* 33 (2004): 643–659; Sarah Kaplan, Fiona Murray, and Rebecca Henderson, "Discontinuities and Senior Management: Assessing the Role of Recognition in Pharmaceutical Firm Response to Biotechnology," *Industrial and Corporate Change* 12 (2003): 203–233; Paul Rabinow, *Making PCR: A Story of Biotechnology* (University of Chicago Press, 1996); Sergio Sismondo, "Pharmaceutical Company Funding and Its Consequences: A Qualitative Systematic Review," *Contemporary Clinical Trials* 29 (2008): 109–113.

25. Colin Norman, "Electronics Firms Plug into Universities," *Science* 217 (August 6, 1982): 511–514.

26. Richard R. Nelson, *Technology, Institutions, and Economic Growth* (Harvard University Press, 2005), pp. 153 and 32–33.

27. The canonical first statement of the law is usually quoted from Gordon E. Moore, "Cramming More Components onto Integrated Circuits," *Electronics* 38.8

(1965): 114–117. Moore articulated the shift from a 12-month period to two years in "Progress in Digital Electronics," *International Electron Devices Meeting* 21 (1975): 11–13. For good overviews of the complicated history of Moore's Law, see Ethan Mollick, "Establishing Moore's Law," *IEEE Annals of the History of Computing* 28.3 (2006): 62–75 and David C. Brock (ed.), *Understanding Moore's Law: Four Decades of Innovation* (Chemical Heritage Foundation, 2006).

28. Gordon Moore, "Moore's Law at 40," in *Understanding Moore's Law*, ed. Brock.

29. One-off (rather than commercially mass-produced) transistors can be even smaller. See John Markoff, "Physicists Create a Working Transistor from a Single Atom," *New York Times*, February 19, 2012.

30. Lécuyer, *Making Silicon Valley*.

31. Arthur L. Robinson, "Giant Corporations from Tiny Chips Grow," *Science* 208 (May 2, 1980): 480–484.

32. Clayton M. Christensen, Steven King, Matt Verlinden, and Woodward Yang, "The New Economics of Semiconductor Manufacturing," *IEEE Spectrum* 45.5 (2008): 24–29.

33. Mara Mills, "Hearing Aids and the History of Electronics Miniaturization," *IEEE Annals of the History of Computing* 33.2 (2011): 24–45; Eric S. Hintz, "Portable Power: Inventor Samuel Ruben and the Birth of Duracell," *Technology and Culture* 50.1 (2009): 24–57; Michael Aaron Dennis, "'Our First Line of Defense': Two University Laboratories in the Postwar American State," *Isis* 85 (1994): 427–455; Albert Glinsky, *Theremin: Ether Music and Espionage* (University of Illinois Press, 2000).

34. Hyungsub Choi, Manufacturing Knowledge in Transit: Technical Practice, Organizational Change, and the Rise of the Semiconductor Industry in the United States and Japan, 1948–1960, PhD dissertation, Johns Hopkins University, 2007; Christophe Lécuyer and David C. Brock, *Makers of the Microchip: A Documentary History of Fairchild Semiconductor* (MIT Press, 2010).

35. Gordon E. Moore, "No Exponential Is Forever: But 'Forever' Can Be Delayed!" in *2003 IEEE International Solid-State Circuits Conference* (IEEE, 2003).

36. Randal Goodall et al., "Long-Term Productivity Mechanisms of the Semiconductor Industry," in *Proceedings of the Ninth International Symposium on Silicon Materials Science and Technology*, ed. H. R. Huff, L. Fabry, and S. Kishino (Electrochemical Society, 2002).

37. Mollick, "Establishing Moore's Law."

38. Brock, *Understanding Moore's Law*; Donald MacKenzie, *Knowing Machines: Essays on Technical Change* (MIT Press, 1996); David C. Brock and Christophe Lécuyer, "Digital Foundations: The Making of Silicon-Gate Manufacturing Technology,"

Technology and Culture 53 (2012): 561–597; W. Patrick McCray, “From Lab to iPod: A Story of Discovery and Commercialization in the Post–Cold War Era,” *Technology and Culture* 50 (2009): 58–81; Donald MacKenzie, *An Engine, Not a Camera: How Financial Models Shape Markets* (MIT Press, 2006).

39. Kevin Kelly, “Was Moore’s Law Inevitable?” The Technium blog, July 17, 2009 (http://kk.org/thetechnium/archives/2009/07/was_moores_law.php). For a similar but more quantitative analysis, see Heebyung Kim and Christopher L. Magee, “A Functional Approach for Studying Technological Progress: Application to Information Technology,” *Technological Forecasting and Social Change* 73 (2006): 1061–1083.

40. Paul E. Ceruzzi, “Moore’s Law and Technological Determinism: Reflections on the History of Technology,” *Technology and Culture* 46 (2005): 584–594.

41. John Searle, *The Construction of Social Reality* (Simon and Schuster, 1995); Anthony Giddens, *New Rules of Sociological Method: A Positive Critique of Interpretive Sociologies* (Basic Books, 1975). As Giddens’ title implies, the “social facts” literature generally looks back to Emile Durkheim’s 1895 book *Les Règles de la Méthode Sociologique*.

42. Jonathan G. Koomey et al., “Implications of Historical Trends in the Electrical Efficiency of Computing,” *IEEE Annals of the History of Computing* 33.3 (2011): 46–54; John Markoff, “Progress Hits Snag: Tiny Chips Use Outsize Power,” *New York Times*, July 31, 2011. There is already a “Gene’s Law” (named for Texas Instruments’ Gene Frantz), which says that “the power necessary to perform the [multiply accumulate] instruction … was being reduced at the rate of half the power every 18 months.” Gene Frantz, “Signal Core: A Short History of the Digital Signal Processor,” *IEEE Solid-State Circuits* 4.2 (2012): 16–20.

43. Hans Moravec, *ROBOT: Mere Machine to Transcendent Mind* (Oxford University Press, 1999); Ray Kurzweil, *The Singularity Is Near* (Penguin, 2005).

44. Ceruzzi observes in “Moore’s Law and Technological Determinism” that “the moment of its [Moore’s Law’s] demise seems always to be about ten years from whenever the prediction is made.” To some extent, the ten-year rule is now enshrined officially, in that the International Technological Roadmap for Semiconductors’ planning horizon is of that order—see Paul S. Peercy, “The Drive to Miniaturization,” *Nature* 406 (August 31, 2000): 1023–1026.

45. Hyungsub Choi has found that as early as 1958 the Air Force was confident that conventional semiconductor integrated circuit technology would hit a miniaturization “asymptote” by the early 1960s, and that therefore some as-yet-untried technology would soon have to replace “present techniques”; see Hyungsub Choi and Cyrus C. M. Mody, “The Long History of Molecular Electronics: Microelectronics Origins of Nanotechnology,” *Social Studies of Science* 39 (2009): 11–50. For a present-day version of the same argument, see John Markoff, “After the Transistor, a Leap into the Microcosm,” *New York Times*, August 31, 2009. An influential pessimistic

assessment of miniaturization that will figure prominently in my narrative is Robert W. Keyes, "Physical Limits in Digital Electronics," *Proceedings of the IEEE* 63.5 (1975): 740–767.

46. Peter A. Shulman, "'Science Can Never Demobilize': The United States Navy and Petroleum Geology, 1898–1924," *History and Technology*, 19 (2003): 365–395.

47. Andrew "bunnie" Huang, "The Death of Moore's Law Will Spur Innovation," spectrum.ieee.org/semiconductors/design/the-death-of-moores-law-will-spur-innovation.

48. Rose Marie Ham, Greg Linden, and Melissa M. Appleyard, "The Evolving Role of Semiconductor Consortia in the United States and Japan," *California Management Review* 41 (1998): 137–163; Larry D. Browning, Janice M. Beyer, and Judy C. Shetler, "Building Cooperation in a Competitive Industry: SEMATECH and the Semiconductor Industry," *Academy of Management Journal* 38 (1995): 113–151; Hyungsub Choi, "The Long Tail of the Third Industrial Revolution: Technology Platform and Supply Chain Relationships at SEMATECH," presentation at annual meeting of the Society for the History of Technology, Pittsburgh, 2009.

49. Ross Knox Bassett, *To the Digital Age: Research Labs, Start-Up Companies, and the Rise of MOS Technology* (Johns Hopkins University Press, 2002); Leslie Berlin, *The Man behind the Microchip: Robert Noyce and the Invention of Silicon Valley* (Oxford University Press, 2005); Lécuyer, *Making Silicon Valley*; Lécuyer and Brock, *Makers of the Microchip*.

50. Gordon Moore, "Entrepreneur Dinner Speech" at dinner after groundbreaking for Moore Laboratory of Engineering, March 8, 1994, transcript available in Historical File S3.2.1, Caltech Archives, California Institute of Technology.

51. Henry Etzkowitz, *The Triple Helix: University-Industry-Government Innovation in Action* (Routledge, 2008).

52. Thomas J. Misa, "Military Needs, Commercial Realities, and the Development of the Transistor, 1948–1958," in *Military Enterprise and Technological Change: Perspectives on the American Experience*, ed. Merritt Roe Smith (MIT Press, 1985); Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (Columbia University Press, 1993).

53. Jonathan Zeitlin, "The Historical Alternatives Approach," in *Oxford Handbook of Business History*, ed. Geoffrey Jones and Jonathan Zeitlin (Oxford University Press, 2007).

54. The "shadow" metaphor is from Finn Brunton, *Spam: A Shadow History of the Internet* (MIT Press, 2013).

55. See, e.g., Bassett, *To the Digital Age: Research Labs*; Berlin, *The Man behind the Microchip*; Lécuyer, *Making Silicon Valley*; Lécuyer and Brock, *Makers of the Microchip*;

Choi, Manufacturing Knowledge in Transit; Daniel Holbrook, Wesley M. Cohen, David A. Hounshell, and Steven Klepper, "The Nature, Sources, and Consequences of Firm Differences in the Early History of the Semiconductor Industry," *Strategic Management Journal* 21.10–11 (2000): 1017–1041; Michael Riordan and Lillian Hoddeson, *Crystal Fire: The Invention of the Transistor and the Birth of the Information Age* (Norton, 1997); Martin Kenney (ed.), *Understanding Silicon Valley: The Anatomy of an Entrepreneurial Region* (Stanford University Press, 2000); AnnaLee Saxenian, *Regional Advantage: Culture and Competition in Silicon Valley and Route 128* (Harvard University Press, 1994); Rebecca Henderson, "Underinvestment and Incompetence as Responses to Radical Innovation: Evidence from the Photographic Alignment Equipment Industry," *RAND Journal of Economics* 24 (1993): 248–270; Nile W. Hatch and David C. Mowery, "Process Innovation and Learning by Doing in Semiconductor Manufacturing," 44 (1998): 1461–1477.

56. On federal labs, my work is indebted to Peter J. Westwick, *The National Labs: Science in an American System, 1947–1974* (Harvard University Press, 2003); on universities, to Leslie, *The Cold War and American Science*; on federal funding agencies, to Daniel Lee Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States* (Duke University Press, 1995); on corporate research, to Glen Ross Asner, The Cold War and American Industrial Research, PhD dissertation, Carnegie Mellon University, 2006; on philanthropies and social science, to Hunter Crowther-Heyck, "Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Sciences," *Isis* 97 (2006): 420–446.

57. Thomas J. Misa, "Retrieving Sociotechnical Change from Technological Determinism," in *Does Technology Drive History? The Dilemma of Technological Determinism*, ed. Merritt Roe Smith and Leo Marx (MIT Press, 1994).

58. Letter from James Meindl to Erich Bloch, May 10, 1985, National Archives and Records Administration II, National Science Foundation, NSF Historian collection, 307–130–37–16-(1–6), Box 47, Folder 5–23–85 Ctr. For Integrated Systems Stanford Univ.

59. Remarks of Mr. Erich Bloch, Director NSF, at the dedication of the center building, Center for Integrated Systems, Stanford University, May 23, 1985, National Archives and Records Administration II, National Science Foundation, NSF Historian collection, 307–130–37–16-(1–6), Box 47, Folder 5–23–85 Stanford Univ.

60. Josh Whitford and Andrew Schrank, "The Paradox of the Weak State Revisited: Industrial Policy, Network Governance, and Political Decentralization," in *State of Innovation: The US Government's Role in Technology Development*, ed. Fred Block and Matthew R. Keller (Paradigm, 2011).

61. Paul J. DiMaggio, "Constructing an Organizational Field as a Professional Project: US Art Museums, 1920–1940," in *The New Institutionalism in Organizational Analysis*, ed. Walter W. Powell and Paul J. DiMaggio (University of Chicago Press, 1991).

Chapter 1

1. Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (Columbia University Press, 1993); Rebecca Lowen, *Creating the Cold War University: The Transformation of Stanford* (University of California Press, 1997); Eric J. Vettel, *Biotech: The Countercultural Origins of an Industry* (Philadelphia, 2006); C. Stewart Gillmore, *Fred Terman at Stanford: Building a Discipline, a University, and Silicon Valley* (Stanford University Press, 2004).

2. Carol Gruber, "The Overhead System in Government-Sponsored Academic Science: Origins and Early Development," *Historical Studies in the Physical Sciences* 25.2 (1995): 241–268.

3. There is a historiographic debate as to whether Vannevar Bush should be given credit for the linear model, and even whether there was such a thing. See, for instance, David Edgerton, "The 'Linear Model' Did Not Exist: Reflections on the History and Historiography of Science and Research in Industry in the Twentieth Century," in *The Science-Industry Nexus: History, Policy, Implications*, ed. Karl Grandin, Nina Wormbs, and Sven Widmalm (Science History Publications, 2004). The emerging consensus is that "Bush's" linear model should probably be credited to Paul Samuelson and other economists—see Benoît Godin, "The Linear Model of Innovation: The Historical Construction of an Analytical Framework," *Science, Technology and Human Values* 31 (2006): 639–67; Philip Mirowski, *Science-Mart: Privatizing American Science* (Harvard University Press, 2011). In some contexts (e.g., the US tax code and the Department of Defense's accounting rubric) its existence was quite palpable—see Glen Ross Asner, The Cold War and American Industrial Research, PhD dissertation, Carnegie Mellon University, 2006. I'm indebted to Rebecca Slayton for articulating the point (with reference back to Lowen) that Terman and other university administrators favored basic research partly out of fear "that applied engineering projects might leave them on the losing end of a competition with industry for military dollars and scientific talent." See Rebecca Slayton, "From a 'Dead Albatross' to Lincoln Labs: Applied Research and the Making of a Normal Cold War University," *Historical Studies in the Natural Sciences* 42.4 (2012): 255–282.

4. Kelly Moore, *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945–1975* (Princeton University Press, 2008); Alondra Nelson, *Body and Soul: The Black Panther Party and the Fight against Medical Discrimination* (University of Minnesota Press, 2011).

5. Arthur P. Stern, "The International Solid State Circuits Community and Its Annual Conference—Genesis and First Decade," in *2003 IEEE Solid State Circuits Conference* (IEEE/S^2 Digital Publishing Group, 2003); Gillmore, *Fred Terman*.

6. John G. Linvill, The Optacon (Research and Development of Tactile Facsimile Reading Aid for the Blind), final report to US Department of Health, Education, and

Welfare/Office of Education/Bureau of Education for the Handicapped, Grant No. OEG-0–8–071112–2995, March 1973.

7. “The Optacon Illustrates Research Objectives at Stanford,” *Stanford Engineering News* no. 71 (January 1970): 1–3. A mark of Meindl’s friendship with the Linvill family is that he also has a daughter named Candace.

8. Ibid.

9. Ibid.

10. Susan McWilliams, *Celebrating 100 Years: Research and Scholarship at Stanford* (Office of the Dean of Research, Stanford University, 1991).

11. *Stanford Engineering News*, 1970.

12. David Kaiser, “Cold War Requisitions, Scientific Manpower, and the Production of American Physicists after World War II,” *Historical Studies in the Physical Sciences* 33.1 (2002): 131–159.

13. *Stanford Engineering News*, 1970; Linvill, *The Optacon*.

14. *Stanford Engineering News*, 1970.

15. Ibid.

16. Ibid.; Christophe Lécuyer, “What Do Universities Really Owe Industry?” *Minerva* 43.1 (2005): 51–71; Lécuyer, “Silicon for Industry: Component Design, Mass Production, and the Move to Commercial Markets at Fairchild Semiconductor, 1960–1967,” *History and Technology* 16.2 (1999): 179–216.

17. Linvill, “The Optacon,” pp. 7 and 13.

18. Ross Knox Bassett, *To the Digital Age: Research Labs, Start-Up Companies, and the Rise of MOS Technology* (Johns Hopkins University Press, 2002).

19. *Stanford University Electronics Research Review* (Stanford Electronics Laboratories, 1967), p. 39.

20. Meindl reviews circuits for the helmet communicator application in James D. Meindl, *Micropower Circuits* (Wiley, 1969).

21. *Stanford University Electronics Research Review* (Stanford Electronics Laboratories, 1970), p. 4.

22. For an overview, see the projects abstracted in *Stanford University Electronics Research Review* (Stanford Electronics Laboratories, 1971).

23. Andrew J. Nelson, “Cacophony or Harmony? Multivocal Logics and Technology Licensing by the Stanford University Department of Music,” *Industrial and Corporate Change* 14 (2005): 93–118.

24. Lowen, *Creating the Cold War University*; Vettel, *Biotech*; Leslie, *Cold War and American Science*; Stuart W. Leslie, "Time of Troubles for the Special Laboratories," in *Becoming MIT: Moments of Decision*, ed. David Kaiser (MIT Press, 2010); Matthew M. Wisnioski, "Inside 'the System': Engineers, Scientists, and the Boundaries of Social Protest in the Long 1960s," *History and Technology* 19 (2003): 313–33; Dorothy Nelkin, *The University and Military Research: Moral Politics at MIT* (Cornell University Press, 1972). For a balanced, if obviously partial, perspective from Stanford's president during the Vietnam-era upheavals, see Richard W. Lyman, *Stanford in Turmoil: Campus Unrest, 1966–1972* (Stanford General Books, 2009).

25. National Science Foundation, *Survey of Research and Development Expenditures at University and Colleges* (National Science Foundation, 2007).

26. See historical tables in *Budget of the United States Government FY 2009* (Office of Management and Budget, 2008).

27. Kaiser, "Cold War Requisitions."

28. Memo from John G. Linvill, executive head, Department of Electrical Engineering, to Committee for the Study of Stanford's Educational Program, re: Comments of Electrical Engineering on Stanford's Educational Programs and Objectives, January 26, 1967, in C. F. Quate Papers (SC0347), Department of Special Collections and University Archives, Stanford University Libraries, Box 4, Binder Electrical Engineering.

29. *Stanford University Electronics Research Review* (Stanford Electronics Laboratories, 1969), p. 1.

30. Stanton A. Glantz et al., D.O.D. Sponsored Research at Stanford, volume 1: Two Perceptions: The Investigator's and the Sponsor's, A Report by a Stanford Workshop on Political and Social Issues (1971), p. 196.

31. Daniel J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942–1945: A Political Interpretation of *Science—The Endless Frontier*," *Isis* 68 (1977): 4–26; Daniel Lee Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States* (Duke University Press, 1995).

32. Dian Belanger, *Enabling American Innovation: Engineering and the National Science Foundation* (Purdue University Press, 1998); Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (University of Chicago Press, 1999).

33. Richard J. Green and Wil Lepkowski, "A Forgotten Model for Purposeful Science," *Issues in Science and Technology* (winter 2006): 69–73.

34. On the growth of engineering science at NSF, see Belanger, *Enabling American Innovation*. On social sciences, see Hunter Crowther-Heyck, "Patrons of the Revolution: Ideals and Institutions in Postwar Behavioral Sciences," *Isis* 97 (2006): 420–446.

35. Memo from Linvill to members of the Senate of the Academic Council, re: Present and Projected Research in Electrical Engineering at Stanford, June 17, 1970, in Richard W. Lyman Papers (SC0215), Department of Special Collections and University Archives, Stanford University Libraries, Box 65, Folder Electrical Engineering.

36. Ibid.

37. Ibid.

38. Integrated Circuits Laboratory brochure, "Integrated Circuits Technology—Opportunities for Graduate Study at Stanford University," September 1973, in Department of Special Collections and University Archives, Stanford University Libraries, collection 3120/4 Electronics Labs.

39. David C. Mowery, Richard R. Nelson, Bhaven N. Sampat, and Arvids A. Ziedonis, *Ivory Tower and Industrial Innovation: University-Industry Technology Transfer before and after the Bayh-Dole Act in the United States* (Stanford Business Books, 2004); Elizabeth Popp Berman, *Creating the Market University: How Academic Science Became an Economic Engine* (Princeton University Press, 2012).

40. Jeannette Colyvas, From Divergent Meanings to Common Practices: Institutionalization Processes and the Commercialization of University Research, PhD dissertation, Stanford University, 2007; Sally Smith Hughes, "Making Dollars out of DNA: The First Major Patent in Biotechnology and the Commercialization of Molecular Biology, 1974–1980," *Isis* 92 (2001): 541–575; Doogab Yi, The Recombinant University: Genetic Engineering and the Emergence of Biotechnology at Stanford, 1959–1980, PhD dissertation, Princeton University, 2008.

41. Letter from Niels J. Reimers to John H. Raubitschek, Assistant to the General Counsel, National Science Foundation, April 9, 1975, in Richard W. Lyman Papers (SC0215), Department of Special Collections and University Archives, Stanford University Libraries, Box 125, Folder "National Science Foundation."

42. Quoted on pp. 69–70 of Colyvas, From Divergent Meanings to Common Practices.

43. Nelson, "Cacophony or Harmony?"

44. Cyrus C. M. Mody and Andrew J. Nelson, "'A Towering Virtue of Necessity': Computer Music at Vietnam-Era Stanford," *Osiris* 28.1 (2013): 254–277.

45. Linvill's first Stanford patent was John G. Linvill, United State Patent no. 3229387, "Reading Aid for the Blind," filed January 14, 1964, granted January 18, 1966. Meindl's first patent was James D. Meindl and Charles F. Hottinger, United States Patent no. 3888238, "Ultrasonic Blood Vessel Imaging System and Method," filed September 28, 1973, granted June 10, 1975.

46. Telesensory Systems business plan, August 21, 1970, author's collection.

47. J. S. Brugler, memorial statement for John Linvill, 2011, author's collection.

48. Ibid.

49. Linvill, “The Optacon.”

50. Ibid.

51. Brugler, memorial statement.

52. Letter from Linvill to Pres. Lyman and other upper administrators (plus Meindl), February 7, 1973, Re: Two Requests of Investigator to Administration Regarding the Lapse or Cessation of the Optacon Project, Richard W. Lyman Papers (SC0215), Department of Special Collections and University Archives, Stanford University Libraries, Box 125, Folder “H.E.W. Office of Education.”

53. Linvill, “The Optacon”; and John G. Linvill, “Alumni Profile,” *RLE Currents* 8.1 (1996): 4–5.

54. Linvill, “The Optacon.”

55. McWilliams, *Celebrating 100 Years*.

56. “Research Exposition Is Planned; Organizational Meeting Tomorrow,” *Campus Report*, December 9, 1970.

57. “Research Exposition Is Planned,” *Campus Report*, January 27, 1971.

58. David Kaiser, *How the Hippies Saved Physics: Science, Counterculture, and the Quantum Revival* (Norton, 2012), chapter 7.

59. Linvill to Lyman, February 7, 1973, re: Two Requests.

60. Ibid.

61. Linvill, “The Optacon.”

62. McWilliams, *Celebrating 100 Years*.

63. “The Optacon Illustrates Research Objectives at Stanford.”

64. Lécuyer, “What Do Universities Really Owe Industry?”

65. Letter from James B. Angell to Pat Devaney, re: RDF Report, January 14, 1976, in Richard W. Lyman Papers (SC0215), Department of Special Collections and University Archives, Stanford University Libraries, Box 23, Folder “National Science Foundation March 1976–March 1977.”

66. Ad reprinted in Linvill, “The Optacon.”

67. Linvill, “Alumni Profile.”

68. Ibid.

69. Quoted in Gene Bylinsky, “How Intel Won Its Bet on Memory Chips,” *Fortune*, November 1973: 143–144.

Chapter 2

1. Standard explanations of conventional solid-state electronics often invoke another carrier, the "hole." A hole can be thought of as a deficit of one electron that moves through a crystal more or less like an electron with a positive charge (real electrons are assigned a negative charge).

2. For a useful overview of the history and theory of superconductivity, see Stephen Blundell, *Superconductivity: A Very Short Introduction* (Oxford University Press, 2009).

3. Bardeen, Cooper, and Schrieffer were awarded the 1972 Nobel Prize in physics for BCS theory. The next year, Ivar Giaever, Brian Josephson, and Leo Esaki shared the physics prize for their work on electron tunneling—in Josephson and Giaever's case, tunneling in superconductors. See Ivar Giaever, "Electron Tunneling and Superconductivity," *Reviews of Modern Physics* 46.2 (1974): 245–250. For an overview of the history of applied superconductivity from the Ford team's perspective, see Arnold Silver, "Superconductivity in Electronics," *IEEE Transactions on Applied Superconductivity* 7.2 (1997): 69–79.

4. Philip W. Anderson, "How Josephson Discovered His Effect," *Physics Today* 23.11 (1970): 23–29.

5. Ibid.

6. Ann Johnson, "How Ford Invented the SQUID," *IEEE Spectrum* 51.11 (2014): 40–61.

7. Scott G. Knowles and Stuart W. Leslie, "'Industrial Versailles': Eero Saarinen's Corporate Campuses for GM, IBM, and AT&T," *Isis* 92.1 (2001): 1–33; Stuart W. Leslie, "Spaces for the Space Age: William Pereira's Aerospace Modernism," in *Blue Sky Metropolis: Aerospace and Southern California*, ed. Peter J. Westwick (University of California Press, 2012).

8. On Garwin's work on gravitational radiation, see Harry Collins, *Gravity's Shadow* (University of Chicago Press, 2004). On Penzias' work on the cosmic background radiation, see Helge Kragh, *Cosmology and Controversy: The Historical Development of Two Theories of the Universe* (Princeton University Press, 1996).

9. On the large amount of basic research done in mid-century American corporate laboratories and the various justifications executives at those firms offered for doing basic research, see Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (University of Chicago Press, 2008); Glen R. Asner, Managing Cold War Science: Government Policy, Corporate Strategy, and Industrial Research, 1945–1972, PhD dissertation, Carnegie Mellon University, 2003.

10. For a participant's history that makes this claim, see John Bremer, "The Invention of a Superconducting Integrated Circuit," *IEEE History Center Newsletter* 75

(November 2007): 6–7. My thanks to David Brock for bringing this argument to my attention. Brock himself only claims that in the early 1960s "cryotron microcircuitry had *surpassed* silicon microchips in its level of integration." David C. Brock, "Dudley Buck and the Computer That Never Was," *IEEE Spectrum* 51.4 (2014): 54–69.

11. David Brock and David Laws ascribe the invention of e-beam lithography to Kenneth Shoulders, whom they call "a close colleague of" Dudley Buck, the inventor of the cryotron. See David C. Brock and David A. Laws, "The Early History of Microcircuitry: An Overview," *IEEE Annals of the History of Computing* 34.1 (2012): 7–19. The memory of Buck's leadership in miniaturization, not just in superconducting electronics, has been kept alive by the MIT Submicron Structures Laboratory and its successors (which we will encounter in chapters 4–6). See "Back to the Future: Professor Dudley A. Buck (1927–1959)," *RLE Currents* 2.1 (December 1988): 18–19; also see transcript of oral-history interview with Henry I. Smith conducted by the author, October 25, 2005 (available from Chemical Heritage Foundation).

12. Transcript of interview with Richard Garwin conducted by Finn Aaserud, June 24, 1991, Niels Bohr Library & Archives, American Institute of Physics, College Park, Maryland (http://www.aip.org/history/ohilist/5075.html).

13. Bremer, "The Invention of a Superconducting Integrated Circuit."

14. The description of the very earliest days of the IBM Josephson program in this and the following two paragraphs comes from interviews with several participants in the program who wished to remain anonymous.

15. Juri Matisoo, "A Cryotron-Like Logic Element Utilizing Weakly-Coupled Superconductors," IBM Invention Disclosure, April 29, 1965, contained in IBM Research notebook 11653, author's collection.

16. George Baron, patent attorney, Yorktown Patent Operations [IBM Research], patentability report on Disclosure 103, May 13, 1965, contained in IBM notebook 11653.

17. Source: interviews with participants in Josephson program.

18. See Asner, Managing Cold War Science, but also see Benoît Godin, "The Linear Model of Innovation: The Historical Construction of an Analytical Framework," *Science, Technology and Human Values* 31 (2006): 639–667 and Philip Mirowski, *Science-Mart: Privatizing American Science* (Harvard University Press, 2011). For a provocative argument that big business supported the linear model to secure tax breaks for the kinds of basic research that small businesses could not support, see Daniel Lee Kleinman, "Layers of Interests, Layers of Influence: Business and the Genesis of the National Science Foundation," *Science, Technology & Human Values* 19.3 (1994): 259–282.

19. That there *is* such a spectrum can, of course, be contested, but hardly anyone was doing so at the time. Harvey Brooks, a Harvard engineer and an influential

figure in science policy in the 1960s and the 1970s, summarized the consensus view thus: “there is a clearly a spectrum of activities ranging from pure research on the one hand to technological development on the other, and to some extent one can locate research activities within this spectrum according to their ‘applied-ness.’” Brooks, “Applied Science and Technological Progress,” *Science* 156.3783 (1967): 1706–1712.

20. Wilhelm Anacker, “Josephson Computer Technology: An IBM Research Project,” *IBM Journal of Research and Development* 24.2 (1980): 107–112.

21. Interview with John Pinkston conducted by the author, June 19, 2012.

22. Seth Lloyd, “Obituary: Rolf Landauer (1927–99): Head and Heart of Physics of Information,” *Nature* 400 (August 19, 1999): 720.

23. Charles H. Bennett and Alan B. Fowler, “Rolf W. Landauer (1927–1999),” in *Biographical Memoirs of the National Academy of Sciences* (National Academy of Sciences, 2009). Presumably Bennett and Fowler believe Bell Labs shared preeminence with IBM Yorktown.

24. Jonathan Zeitlin, “The Historical Alternatives Approach,” in *Oxford Handbook of Business History*, ed. Geoffrey Jones and Jonathan Zeitlin (Oxford University Press, 2007).

25. Source: anonymous interview.

26. Bennett and Fowler, “Rolf W. Landauer.”

27. Source: anonymous interview.

28. Robert W. Keyes, “Physical Problems and Limits in Computer Logic,” *IEEE Spectrum* 6 (May 1969): 36–45; Keyes, Physical Limits in Digital Electronics,” *Proceedings of the IEEE* 63.5 (1975): 740–767.

29. Chris Toumey, “Reading Feynman into Nanotechnology: A Text for a New Science,” *Techné* 12.3 (2008): 133–168.

30. See Fernando Elichirigoity, *Planet Management: Limits to Growth, Computer Simulation, and the Emergence of Global Spaces* (Northwestern University Press, 1999); Michael Egan, *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism* (MIT Press, 2009).

31. Robert N. Noyce, “Large Scale Integration: What Is Yet to Come,” *Science* 195 (1977): 1103–1105; B. Hoeneisen and C. A. Mead, “Fundamental Limitations in Microelectronics—1. MOS Technology,” *Solid-State Electronics* 15.7 (1972): 819–829.

32. W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and a Limitless Future* (Princeton University Press, 2013).

33. Leslie Berlin describes Noyce recalling sharing a stage with Paul Ehrlich in the early 1970s: “Noyce had been angry and hurt to realize that most of the students in

the audience considered himself and his colleagues in technology-based industries 'bad people.' He explained a few years later, 'That's what scared me. [We were] bad compared to Paul Ehrlich, who was essentially arguing we should have zero progress from here on out. … How ridiculous can you be?" Leslie Berlin, *The Man behind the Microchip: Robert Noyce and the Invention of Silicon Valley* (Oxford University Press, 2005), p. 214.

34. Rebecca Henderson, "Of Life Cycles Real and Imaginary: The Unexpectedly Long Old Age of Optical Lithography," *Research Policy* 24.4 (1995): 631–643; W. Patrick McCray, "MBE Deserves a Place in the History Books," *Nature Nanotechnology* 2.5 (2007): 259–261.

35. Keyes, "Physical Limits in Digital Electronics," p. 760.

36. "IBM Tests Computer Units Said to Be Faster and Cooler," *New York Times*, February 16, 1978.

37. This paragraph and the following two are based largely on my interview of Pinkston.

38. The figure of 125 is from Joseph C. Logue, "From Vacuum Tubes to Very Large Scale Integration: A Personal Memoir," *IEEE Annals of the History of Computing* 20.3 (1998): 55–68. Arthur Robinson offers two different estimates: 140 (from 1982) and 115 (from 1983). Arthur L. Robinson, "New Superconductors for a Supercomputer," *Science* 215.4528 (1982): 40–43; "IBM Drops Superconducting Computer Project," *Science* 222.4623 (1983): 492–494.

39. The $20 million per year figure is from Robinson, "IBM Drops Superconducting Computer Project," and is confirmed by various interviewees. The IBM:NSA funding ratio of 3 to 1 is from the Pinkston interview, which also informs the rest of this paragraph. For a good contemporary window on the NSA's enthusiasm for Josephson computing, see Nancy K. Welker and Fernand D. Bedard, "Digital Josephson Technology—Present and Future," in *AIP Conference Proceedings 44* (American Institute of Physics, 1978).

40. Juri Matisoo, "The Superconducting Computer," *Scientific American* 242.5 (1980): 50–65.

41. Source: anonymous interview.

42. Pugh is also a historian of IBM, known best for *Building IBM: Shaping an Industry and Its Technology* (MIT Press, 1995). Pugh's managerial background which he brought to the Josephson extendibility studies is laid out in *Memories That Shaped an Industry: Decisions Leading to the IBM System/360* (MIT Press, 1984).

43. Emerson Pugh et al., Josephson Extendibility Study, undated but circa June, 1980, in IBM Archives, Box 475, Folder 1 of 8 (#8 in box), 1–1.

44. C. Ho, T. Ning, P. Solomon, "Bipolar Technology," Section Four in Josephson Extendibility Study, 4–1.

45. G. R. Gunther-Mohr, memo to Ralph E. Gomory, June 9, 1980, re: Extendibility Study, in IBM Archives, Box 476, Folder 6 of 8 (#1 in box).

46. Emerson W. Pugh, memo to file, March 19, 1980, re: Present Josephson Schedule/Discussion with W. Anacker on 3–17–80, in IBM Archives, Box 476, Folder 6 of 8 (#1 in box).

47. Logue, "From Vacuum Tubes."

48. Ibid.

49. Robinson, "New Superconductors."

50. Logue, "From Vacuum Tubes."

51. Two anonymous interviewees mentioned the Yorktown fab line. On the general practice of West Coast firms' researchers running fabrication lines to mitigate the gap between research and manufacturing, see Nile W. Hatch and David C. Mowery, "Process Innovation and Learning by Doing in Semiconductor Manufacturing," *Management Science* 44 (1998): 1461–1477. On the tendency of established, especially East Coast microelectronics firms (with a focus on RCA and IBM) to keep their researchers much further from the factory floor than West Coast start-ups (with Intel as the exemplar), see Ross Bassett, *To the Digital Age: Research Labs, Start-Up Companies, and the Rise of MOS Technology* (Johns Hopkins University Press, 2002).

52. Logue, "From Vacuum Tubes."

53. Transcript of interview with Alan Kleinsasser conducted by the author, May 3, 2010. Note that Kleinsasser was not among the team members demanding such a letter. Other team members I interviewed (who wished to remain anonymous) framed the issue of right of return to Yorktown in terms of the "fun" of doing research relative to doing development.

54. Pugh, memo re: Present Josephson Schedule.

55. Emerson Pugh et al., Josephson Extendibility Study, June 1983, in IBM Archives, Box 476, Folder 1 of 4 ('83 study—#4 in box), p. 2.

56. Ibid.

57. Cited in Gordon Moore, "Moore's Law at 40," in *Understanding Moore's Law*, ed. David C. Brock (Chemical Heritage Foundation, 2006).

58. Source: Kleinsasser interview.

59. Pugh et al., Josephson Extendibility Study, p. 21.

60. The $100 million figure is quoted from Louise Kehoe, "Death Knell for Josephson? Why the Giant Turned back from the Junction," *Financial Times*, November 22, 1983, and from Gary Taubes, "Is the Third Time the Charm for a Superconducting Computer?" *Science* 261.5129 (1993): 1670–1671.

61. Source of quotation: Philip H. Abelson and Allen L. Hammond, "The Electronics Revolution," *Science* 195.4283 (1977): 1087–1091. Similar statements can be found in "Superconductors Speed up Computers," *Chemical Week*, December 1, 1976; "Computers: Supercool Solution," *The Economist*, December 8, 1979, "Technologies for the '80s: Fresh Competition for Silicon Chips," *Business Week*, July 6, 1981; "Computer Grand Prix," *The Economist*, April 3, 1982; Stacy V. Jones, "Key Process is Devised for New Electronic Switch," *New York Times*, November 23, 1974.

62. Rolf Landauer, "Advanced Technology and Truth in Advertising," *Physica A* 168 (1990: 75–87.

63. Both IBM's cultivation of the STM and its variants for both direct technological and indirect prestige/research capacity reasons and the ambiguous relationship between the STM and the Josephson program are detailed in Cyrus C. M. Mody, *Instrumental Community: Probe Microscopy and the Path to Nanotechnology* (MIT Press, 2011).

64. In particular, Don Eigler and Erhard Schweizer's use of an STM to position 35 xenon atoms to spell out "IBM." For a contemporary headline, see Malcolm W. Browne, "2 Researchers Spell 'I.B.M.,' Atom by Atom," *New York Times*, April 5, 1990. For a look back at the significance of the IBM image, see Chris Toumey, "35 Atoms That Changed the World," *Nature Nanotechnology* 5 (2010): 239–241.

65. For a participant history, see J. Georg Bednorz and K. Alex Müller, "Perovskite-Type Oxides: The New Approach to High-T_c Superconductivity," *Angewandte Chemie* 100.5 (1988): 757–770. The high-T_c discovery and its aftermath have yet to find their historian, but a sociological treatment can be found in Helga Nowotny and Ulrike Felt, *After the Breakthrough: The Emergence of High-Temperature Superconductivity as a Research Field* (Cambridge University Press, 1997).

66. Bednorz and Müller, "Perovskite-Type Oxides."

67. Source: Kleinsasser interview.

68. A. P. Malozemoff, W.J. Gallagher, R. L. Greene, R. B. Laibowitz, and C. C. Tsuei, "Superconductivity at IBM—A Centennial Review: Part II—Materials and Physics," *IEEE/CSC &ESAS European Superconductivity News Forum*, no. 21, July 2012.

69. The patent for this invention is Mark B. Ketchen, "Integrated Miniature DC SQUID Susceptometer for Measuring Properties of Very Small Samples," United States Patent 4,588,947, filed December 30, 1983, awarded May 13, 1986.

70. The algorithm I have in mind is described in Eric P. Kronstadt, Tushar R. Gheewala, and Sharad P. Gandhi, "Small Instruction Cache Using Branch Target Table to Effect Instruction Prefetch," United States Patent 4,691,277, filed October 24, 1984, awarded September 1, 1987. Gheewala was a member of the Josephson team.

71. W. Patrick McCray, "From Lab to iPod: A Story of Discovery and Commercialization in the Post–Cold War Era," *Technology and Culture* 50.1 (2009): 58–81.

72. Task Group on Superconductive Electronics (Naval Studies Board, Commission on Physical Sciences, Mathematics, and Resources, National Research Council), *Superconductive Electronics* (National Academies Press, 1984).

73. Ibid., p. 1.

74. Wesley R. Iversen, "JJs on an Upswing," *Electronics Week*, February 25, 1985: 28–31.

75. Task Group, Superconductive Electronics, p. 3.

76. Ibid., p. 7. The budget figure is not adjusted for inflation.

77. Robinson, "New Superconductors for a Supercomputer." Even as late as 1989, American reporters were presenting the Japanese program in a similar light. See, e.g., David E. Sanger, "Japan Joins the Superconductor Fray," *New York Times*, January 29, 1989.

78. Tsunenori Sakamoto, "Japanese Superconducting Computer," letter to *Science* 262 (December 17, 1993): 1802–1803.

79. John G. Linvill, Developments in the Establishment of the Center for Integrated Systems at Stanford University: An Interim Report, December 1978, CIS in-house archive, general folder.

80. Josh Whitford and Andrew Schrank, "The Paradox of the Weak State Revisited: Industrial Policy, Network Governance, and Political Decentralization," in *State of Innovation: The US Government's Role in Technology Development*, ed. Fred Block and Matthew R. Keller (Paradigm, 2011).

Chapter 3

1. Ed Constant introduced the concept of "presumptive anomaly" to describe situations in which the predicted failing in the current form of a technology stimulates a shift to a radically different form. This framework roughly explains the appeal of various exotic forms of microelectronics, particularly molecular electronics. Edward Constant, *The Origins of Turbojet Revolution* (Johns Hopkins University Press, 1980).

2. The information on the "tyranny of numbers" problem, and on the responses to it, is from Hyungsub Choi, Manufacturing Knowledge in Transit: Technical Practice, Organizational Change, and the Rise of the Semiconductor Industry in the United States and Japan, 1948–1960, PhD dissertation, Johns Hopkins University, 2007. Choi covers the first generation of molecular electronics in depth in chapter 3.

3. On the role of the “tyranny of numbers” problem in accelerating invention of the integrated circuit, see Michael Riordan and Lillian Hoddeson, *Crystal Fire: The Invention of the Transistor and the Birth of the Information Age* (Norton, 1997); T. R. Reid, *The Chip: How Two Americans Invented the Microchip and Launched a Revolution* (Random House, 1985).

4. Choi, Manufacturing Knowledge in Transit, p. 73ff.

5. On SLT, see Ross Knox Bassett, *To the Digital Age: Research Labs, Start-Up Companies, and the Rise of MOS Technology* (Johns Hopkins University Press, 2007), p. 67ff.

6. David C. Brock, *Patterning the World: The Rise of Chemically Amplified Photoresists* (Chemical Heritage Foundation, 2009).

7. Choi, Manufacturing Knowledge in Transit, p. 83.

8. Arthur von Hippel, “Molecular Engineering,” *Science* 123.3191 (1956): 315–317.

9. Ed Regis, *Nano: The Emerging Science of Nanotechnology* (Little, Brown, 1995).

10. For a rather positive appraisal of the Westinghouse program—somewhat contrary to conventional histories of microelectronics—see Edgar A. Sack and David A. Laws, “Westinghouse: Microcircuit Pioneer from Molecular Electronics to ICs,” *IEEE Annals of the History of Computing* 34.1 (2012): 74–82.

11. Hyungsub Choi and Cyrus C. M. Mody, “The Long History of Molecular Electronics: Microelectronics Origins of Nanotechnology,” *Social Studies of Science* 39 (2009): 11–50.

12. For the 1968 date, see Sack and Laws, “Westinghouse.” For a reference to the USAF Molecular Electronics Branch, see memo from R. W. Olson to Mark Shepherd re: Molecular Electronics Branch of the Electronics Technology Laboratory, Wright-Patterson Air Force Base, Dayton, Ohio, November 27, 1965, in Texas Instruments Records, DeGolyer Library, Southern Methodist University, RG-1, Accession 88–5, Bucy Papers, Box 39E.

13. Oral-history interview with Alan Heeger conducted by the author, March 13 and 16, 2006, available from the Chemical Heritage Foundation oral history collection. See also “Dr. Garito, Physics and Astronomy,” *University of Pennsylvania Almanac* 53.12 (2006).

14. L. B. Coleman et al., “Superconducting Fluctuations and the Peierls Instability in an Organic Solid,” *Solid State Communications* 12.11 (1973): 1125–1132.

15. Heeger oral history.

16. Heeger oral history; oral-history interview with Alan MacDiarmid conducted by the author, December 19, 2005, in Chemical Heritage Foundation oral history collection.

17. Hyungsub Choi and Brittany Shields, “A Place for Materials Research: Laboratory Buildings and Interdisciplinary Research at the University of Pennsylvania,” *Minerva* 53 (2015): 21–42.

18. Cyrus C. M. Mody and Hyungsub Choi, “From Materials Science to Nanotechnology: Institutions, Communities, and Disciplines at Cornell University,” *Historical Studies in the Natural Sciences* 43.2 (2013): 121–161.

19. MacDiarmid oral history.

20. Ibid. See also A. G. MacDiarmid et al., “Supplementary Proposal to Office of Naval Research for Research Contract N00014-75-C-0962 on Synthesis and Study of Polymeric Metals,” February 27, 1976, author’s collection.

21. C. K. Chiang et al., “Electrical Conductivity in Doped Polyacetylene,” *Physical Review Letters* 39.17 (1977): 1098–1101.

22. Heeger oral history.

23. R. L. Green and G. B. Street, “Conducting Organic Materials,” *Science* 226.4675 (1984): 651–656.

24. A. P. Malozemoff, W. J. Gallagher, R. L. Greene, R. B. Laibowitz, and C. C. Tsuei, “Superconductivity at IBM—A Centennial Review: Part II—Materials and Physics,” *IEEE/CSC &ESAS European Superconductivity News Forum*, no. 21 (July 2012).

25. The patent is Edward M. Engler, John D. Kuptsis, Robert G. Schad, and Yaffa Tomkiewicz, “Class of E-Beam Resists Based on Conducting Organic Charge Transfer Salts,” US patent no. 4312935, filed December 8, 1980, granted January 26, 1982. Note the following contemporaneous article: Y. Tomkiewicz, E. M. Engler, B. A . Scott, S. J. LaPlaca, and H. Brom, “Doping Organic Solids—Its Uses to Probe and to Modify Electronic Properties,” *Molecular Metals* [NATO Conference Series] 1 (1979): 43–49. Ari Aviram was also listed as an inventor on a European patent application for a later conductive polymer-based resist: A. Aviram et al., “Photo, E-Beam, And X-Ray Sensitive Negative Resists Based on Donor Polymer-Based Halocarbon Acceptor Transfer Complexes and Method For Producing Negative Resist Images,” European patent application 80105502.1, filed September 15, 1980.

26. Oral-history interview with Mark Ratner conducted by the author, April 7, 2006, available from Chemical Heritage Foundation oral history collection.

27. Mort La Brecque, “Molecular Electronics: Circuits and Devices a Molecule Wide,” *Mosaic* 20.1 (1988): 16–27. Brackets in original.

28. Ratner oral history.

29. Ibid.

30. See, e.g., B. A. Scott, R. Mehran, B. D. Silverman, and M. A. Ratner, “Magnus Green Salt Solutions Containing Mixed-Valence Platinum Chains: An Approach to

1-D Metals," in *Extended Interactions between Metal Ions*, ed. Leonard V. Interrante (American Chemical Society, 1974).

31. "Molecules as Electronic Components: Some Basic Considerations," IBM Archives, Press Release Collection, Research Division, 1974.

32. Ari Aviram, Molecular Rectifiers, PhD dissertation, New York University, 1975.

33. "Molecules as Electronic Components."

34. Arieh Aviram and Mark A. Ratner, "Molecular Rectifiers," *Chemical Physics Letters* 29.2 (1974): 277–283.

35. "Mini-Mini Components," *Time* 103 (May 6, 1974): 97; Arieh Aviram, Marvin Joseph Freiser, Philip Edward Seiden, and William Robert Young, "Organic Electronic Rectifying Devices," US Patent 3953874A, filed March 12, 1974, granted April 27, 1976; Arieh Aviram and Philip E. Seiden, "Organic Memory Device," US Patent 3833894, filed June 20, 1973, granted September 3, 1974.

36. Ratner oral history.

37. Ibid. For an example, see Ari Aviram and Susan O. Ellmann, "Four Layered Ribbon for Electrothermal Printing," US Patent 4400100, filed March 2, 1981, granted August 23, 1983.

38. See, in particular, F. L. Carter, "Problems and Prospects of Future Electroactive Polymers and 'Molecular' Electronic Devices," in *The NRL Program on Electroactive Polymers*, ed. L. B. Lockhart (Naval Research Laboratory, 1979): 121–175; F. L. Carter, "Further Considerations on 'Molecular' Electronic Devices," in *The NRL Program on Electroactive Polymers*, ed. R. B. Fox (Naval Research Laboratory, 1980).

39. Forrest L. Carter, Complexes of the Cuprous Halides with Dialkyl Acteylenes, PhD dissertation, California Institute of Technology, 1956. Carter's friendship with Feynman and other aspects of Carter's colorful personality came up in interviews I conducted with colleagues who wished to remain anonymous.

40. See, for instance, Forrest L. Carter, "The Chemistry in Future Molecular Computers," in *Computer Applications in Chemistry*, ed. Stephen R. Heller and Rudolph Potenzone Jr. (Elsevier, 1983). It is notable, too, that the introductory speech to Carter's first Molecular Electronic Devices conference given by Albert Schindler (the NRL's associate director of research and an occasional research collaborator of Carter's) also refers to the Feynman speech (A. I. Schindler, "Opening Remarks," in *Molecular Electronic Devices*, ed. Forrest L. Carter, Marcel Dekker, 1982: 1–2). Of course, Carter may have fed Schindler the reference. Even so, the NRL appears to have been one of the few places where people were talking about the Feynman speech before 1980.

41. H. Wohltjen, "Forrest L. Carter—A Pioneer in Molecular Electronics," in *Molecular Electronics: Biosensors and Biocomputers*, ed. Felix T. Hong (Plenum, 1989), p. 1.

42. Grant Fjermedal, *The Tomorrow Makers: A Brave New World of Living-Brain Machines* (Macmillan, 1986), p. 169. Kathleen McAuliffe provides my favorite description of Carter: "a tall, bearded man who would look at home in the British Admiralty" ("The Biochip Revolution," *Omni* 4.3, 1981: 52–60). Many publications featuring Carter also included a posed photo.

43. See Forrest L. Carter and Robert Mazelsky, "The ZnSb Structure: A Further Enquiry," *Journal of Physics and Chemistry of Solids* 25.6 (1964): 571–581; F. M. Ryan, I. M. Ryan, F. L. Greenberg, F. L. Carter, and R. C. Miller, "Thermoelectric Properties of Some Cerium Sulfide Semiconductors from 4° to 1300°K [*sic*]," *Journal of Applied Physics* 33 (1962): 864–868.

44. Carter, "Further Considerations."

45. Ibid.

46. Alan Berman, "Talk for 'Molecular' Electronic Devices Workshop," in *Molecular Electronic Devices*, ed. Forrest L. Carter (Marcel Dekker, 1982).

47. Compare Forrest L. Carter, "Conformational Switching at the Molecular Level," in *Molecular Electronic Devices*, ed. Forrest L. Carter (Marcel Dekker, 1982) with A. G. MacDiarmid and A. J. Heeger, "Recent Advances in the Chemistry and Physics of Polyacetylene: Solitons as a Means of Stabilizing Carbonium Ions and Carbanions in Doped $(CH)_x$," in *Molecular Electronic Devices*, ed. Forrest L. Carter (Marcel Dekker, 1982).

48. That Carter believed his consideration of molecular wires was a major contribution that went well beyond Aviram and Ratner can be seen in Carter, "The Chemistry in Future Molecular Computers": "The concept of molecular switching in electronic devices became viable in 1978 at the Naval Research Laboratory when it was recognized that communication with the switching moieties was possible by using molecular 'wires' of linear polymeric conductors. … The enormous implications inherent in the concept resulted in a workshop on the subject in 1981. … Earlier in 1974, Aviram and Ratner had discussed a 'molecular rectifier' but one without 'wires.'"

49. Source: interview with Mark Reed conducted by the author, July 7, 2005.

50. La Brecque, "Circuits and Devices."

51. Noel S. Hush, "An Overview of the First Half-Century of Molecular Electronics," *Annals of the New York Academy of Sciences* 1006 (2003): 1–20.

52. La Brecque, "Circuits and Devices."

53. Felix T. Hong, preface, in *Molecular Electronics: Biosensors and Biocomputers*, ed. Felix T. Hong (Plenum, 1989).

54. Sarah Kaplan and Joanna Radin, "Bounding an Emerging Technology: Para-Scientific Media and the Drexler-Smalley Debate about Nanotechnology," *Social Studies of Science* 41 (2011): 457–485.

55. "Free thinkers" is from Stephanie Yanchinski, "And Now—the Biochip," *New Scientist* 93 (1982): 68–71. See also Michael Conrad, "The Lure of Molecular Computing: While Marketable Products Seem Decades Away, Researchers Are Crystallizing Theories and Devices That Will Give Biological Organisms the Power to Computer," *IEEE Spectrum* 23.10 (1986): 55–60; Mark A. Clarkson, "The Quest for the Molecular Computer," *Byte* 14.5 (1989): 268–273; G. Alan Schick, Albert F. Lawrence, and Robert R. Birge, "Biotechnology and Molecular Computing," *TIBTECH* 6 (July 1988): 159–163.

56. Jeannette A. Colyvas, "Factory, Hazard, and Contamination: The Use of Metaphor in the Commercialization of Recombinant DNA," *Minerva* 45 (2007): 143–159.

57. Andrew Pollack, "Computers from Bacteria," *New York Times*, February 18, 1982.

58. W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and a Limitless Future* (Princeton University Press, 2013).

59. See ibid.; Kaplan and Radin, "Bounding an Emerging Technology"; Cyrus C. M. Mody, *Instrumental Community: Probe Microscopy and the Path to Nanotechnology* (MIT Press, 2011); Chris Toumey, "Tracing and Disputing the Story of Nanotechnology," in *International Handbook on Regulating Nanotechnologies*, ed. Graeme A. Hodge, Diana M. Bowman, and Andrew D. Maynard (Edward Elgar, 2010).

60. Carter, "Further Considerations."

61. Forrest L. Carter, "The Molecular Device Computer: Point of Departure for Large Scale Cellular Automata," in *Cellular Automata*, ed. Doyne Farmer, Tommaso Toffoli, and Stephen Wolfram (North-Holland Physics, 1984). For more on the Santa Fe Institute and its members' intellectual and material synthesis of organic chemistry, computer science, and semiconductor physics—not dissimilar to Carter's project—see Stefan Helmreich, *Silicon Second Life: Culturing Artificial Life in a Digital World* (University of California Press, 1998).

62. Otavio Bueno, "Von Neumann, Self-Reproduction, and the Constitution of Nanophenomena," in *Discovering the Nanoscale*, ed. Davis Baird, Alfred Nordmann, and Joachim Schummer (IOS, 2004).

63. Fjermedal, *The Tomorrow Makers*, pp. 168–169.

64. McAuliffe, "The Biochip Revolution."

65. See Hong, preface. Conrad's "Feasibility Study" is cited as one of the first two surveys of the field in F. Eugene Yates, *Chemically-Based Computer Designs: Report of a Conference on Molecular Computing* (Crump Institute for Medical Engineering, August 1, 1984). I have been unable to find the report, co-authored with Kiumi Akingbehin

and C. Friedlander, but it appears to have been written for National Geno Sciences in 1982.

66. See Brian D. Josephson, "'Beyond Quantum Theory: A Realist Psycho-Biological Interpretation of Reality' Revisited," *BioSystems* 64 [special issue in memory of Michael Conrad] (2002): 43–45. There Josephson describes "some specific ideas on which we [Josephson and Conrad] collaborated" and continues as follows: "One of these was the idea from Eastern Philosophy that, in certain states of consciousness, the *subjective* states of the mind, irrespective of learning, closely reflect *objective* reality. … Such an idea had been discussed by Fritjof Capra in his book *The Tao of Physics*, concerned with the deep parallels that appear to exist between patterns found in objective reality as revealed by modern science, and patterns found in deeper personal experiences as revealed by meditation or mystical experience and reported by the mystics."

67. See S. R. Hameroff and R. C. Watt, "Microtubules: Biological Microprocessors?" in *Molecular Electronic Devices*, ed. Forrest L. Carter (Marcel Dekker, 1982). Like Conrad, Hameroff indicated his interest in quantum consciousness only obliquely in his formal writings on molecular electronics. For a more overt statement, see Hameroff, "Quantum Coherence in Microtubules: A Neural Basis for Emergent Consciousness?" *Journal of Consciousness Studies* 1.1 (1994): 91–118.

68. See, for example, Conrad Schneiker, "Nanotechnology with Feynman Machines: Scanning Tunneling Engineering and Artificial Life," in *Artificial Life*, volume VI: *Santa Fe Institute Studies in the Sciences of Complexity*, ed. C. Langton (Addison-Wesley, 1989). That article is "dedicated to the *original* NanoTechnologist, *Richard P. Feynman* (11 May 1918–15 Feb 1988) and to a major pioneer of *electronic* NanoComputing, *Forrest L. Carter* (29 April 1930–20 Dec 1987)." For a survey of Schneiker's early proposals to bring STM into molecular electronics, see Chris Toumey, "The Man Who Understood the Feynman Machine," *Nature Nanotechnology* 2.1 (2007): 9–10.

69. I'm not implying that Hameroff *is* a believer in paranormal phenomena or that his work on consciousness invokes phenomena outside conventional scientific understandings, though there are some scientists who would claim the latter. Rather, I'm arguing that Hameroff's association with advocates of parapsychology and quantum mysticism (witness, for instance, his featured interviews in the 2005 film *What the Bleep Do We Know!?*) would, for many observers, have reinforced the "louche reputation" of American molecular electronics generally, and of Forrest Carter specifically.

70. Reed interview.

71. Ratner oral history.

72. Arthur L. Robinson, "Nanocomputers from Organic Molecules?" *Science* 220.4600 (1983): 940–942.

73. J. L. Fox, "Molecular Electronic Devices Offer Challenging Goal," *Chemical and Engineering News* 61.21 (1983): 27–29.

74. Rudy Baum, "Molecular Computers Are Far From Realization," *Chemical and Engineering News* 61.46 (1983): 33–34.

75. Edwin A. Chandross, "Molecular Electronics," *Chemical and Engineering News* 61.51 (1983): 2.

76. Richard Jones, "What Has Nanotechnology Taught Us about Contemporary Technoscience?" (presentation at meeting of Society for the Study of Nanoscience and Emerging Technologies, Darmstadt, Germany 2010). Slides from a related presentation with the "louche reputation" line in the notes can be found at www.nottingham.ac.uk/sciencetechnologyandsociety/documents/slidesresponsibleinnovation.pptx.

77. Richard A. L. Jones, "What Has Nanotechnology Taught Us about Contemporary Technoscience?" in *Quantum Engagements: Social Reflections of Nanoscience and Emerging Technologies*, ed. T. Zülsdorf et al. (IOS Press, 2011).

78. My description of Carter's difficult relationship with the NRL's management during his final years comes from interviews with several of his colleagues, including both friends and critics, who wished to remain anonymous.

79. R. M. Metzger et al., "Observation of Unimolecular Electrical Rectification in Hexadecylquinolinium Tricyanoquinodimethanide," *Thin Solid Films* 327–329 (1998): 326–330.

80. Robert M. Metzger, "Electrical Rectification by a Molecule: The Advent of Unimolecular Electronic Devices," *Accounts of Chemical Research* 32 (1999): 950–957.

81. Mathias Grote, "Purple Matter, Membranes, and 'Molecular Pumps' in Rhodopsin Research (1960s–1980s)," *Journal of the History of Biology* 46 (2013): 331–368.

82. See Schick, Lawrence, and Birge, "Biotechnology and Molecular Computing." Also see Felix T. Hong and Michael Conrad, "The Bacteriorhodopsin Membrane as a Prototype Molecular Electronic Device," in *Molecular Electronic Devices*, ed. Forrest L. Carter, Ronald E. Siatkowski, and Hank Wohltjen (North-Holland, 1988).

83. Erica Fuchs has examined in detail DARPA's long-term support for the idea of optical connections in a way that complements my analysis of university-industry-government interactions that were even further toward the margins of the microelectronics industry. See, for instance, Erica R. H. Fuchs, "Rethinking the Role of the State in Technology Development: DARPA and the Case for Embedded Network Governance," *Research Policy* 39.9 (2010): 1133–1147.

84. See, for instance, *Proceedings of the International Symposium on Future Electron Devices—Bioelectronic and Molecular Electronic Devices*, ed. Masuo Aizawa (Research & Development Association for Future Electron Devices, 1985).

85. Toru Watsuji et al., “Introduction to the Bioelectronic Devices Project in Japan,” *Bio Systems* 35 (1995): 101–106.

86. La Brecque, “Circuits and Devices.”

87. Ibid.

88. Robert W. Munn, “Molecules and Electronics in Poland,” in *The Status of Civil Science in Eastern Europe* (NATO, 1989); interview with Robert Munn conducted by the author, July 26, 2013.

89. Michael Conrad, “Introduction—Proceedings of MEBC 93,” *Bio Systems* 35 (1995): 99–100; interview with Michael Petty conducted by the author, July 24, 2013.

90. “Professor Sir Gareth Roberts,” *The Times* (London), March 20, 2007.

91. Cyril Hilsum, “Sir Gareth Gwyn Roberts,” *Biographical Memorials of Fellows of the Royal Society* 57 (2011): 379–393.

92. Ibid.

93. Ibid.

94. Ibid.

95. Molecular Electronics Advisory Group, *The Report of the Molecular Electronics Advisory Group on a Strategy for the Support of Molecular Electronics Research, to the Science and Engineering Research Council*, 1985.

96. Geoffrey Pattie [Minister of State for Information and Technology], “Research: Ingenuity to Prosperity,” *Electronics and Power*, January 1987: 25–26. The specific amount can be found in “Launching LINK,” *ME News* 1 (summer 1988): 1.

97. Molecular Electronics Advisory Group, *Report*.

98. Ibid.

99. Memo from W. J. Feast, re: Molecular Electronics and Durham University Report and Comment, October 1986, Durham University Records: Central Administration and Officers, Pro-Vice-Chancellor’s records, collection UND/CP1/58, Durham University Library Special Collections.

100. Ibid.

101. Molecular Electronics Advisory Group, *Report*.

102. Memo from Mike Petty, re: Annual Report of Centre for Molecular Electronics (March 1, 1988 to February 28, 1989), Durham University Records: Central Administration and Officers, University Office records, collection UND/CB1/M5a, Durham University Library Special Collections.

103. Ibid.

104. Anne Marcovich and Terry Shinn, *Toward a New Dimension: Exploring the Nanoscale* (Oxford University Press, 2014).

Chapter 4

1. Dian Belanger, *Enabling American Innovation: Engineering and the National Science Foundation* (Purdue University Press, 1998); Juan C. Lucena, *Defending the Nation: US Policymaking to Create Scientists and Engineers from Sputnik to the 'War against Terrorism'* (University Press of America, 2005); Daniel Lee Kleinman, *Politics on the Endless Frontier: Postwar Research Policy in the United States* (Duke University Press, 1995); Bruce L. R. Smith, *American Science Policy since World War II* (Brookings Institution, 1990).

2. Blanchard Hiatt, "The Great Astronomical Ear," *Mosaic* 11.2 (1980): 30–37.

3. Arthur Fisher, "The Magnetism of a Shared Facility," *Mosaic* 11.2 (1980): 38–45. Jay Harris, in particular, invoked the Bitter Lab as a model for the National Research and Resource Facility for Submicron Structures. See oral-history interview with Henry I. Smith conducted by the author, October 25, 2005, available from the Chemical Heritage Foundation.

4. Olof Hallonsten, Small Science on Big Machines: Politics and Practices of Synchrotron Radiation Laboratories, PhD dissertation, Lund University, 2009.

5. Toby A. Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945–1975* (Johns Hopkins University Press, 2000).

6. William J. Cromie, "Regional Instrumentation Centers," *Mosaic* 11.2 (1980): 12–18.

7. Charles Polk, "Address to the NSF Workshop," in *Report of the NSF Workshop on Needs for a National Research and Resource Center in Submicron Structures (East Coast)*, submitted by J. N. Zemel and M. S. Chang (Moore School of Electrical Engineering, University of Pennsylvania, May 10, 1976).

8. Mark L. Schattenburg, "History of the 'Three Beams' Conference, the Birth of the Information Age, and the Era of Lithography Wars," 2007 (http://eipbn.org/2010/wp-content/uploads/2010/01/EIPBN_history.pdf).

9. Ibid.

10. Interview with Alan Kleinsasser conducted by the author, May 3, 2010.

11. Leslie Berlin, *The Man behind the Microchip: Robert Noyce and the Invention of Silicon Valley* (Oxford University Press, 2005).

12. Rebecca Henderson, "Of Life Cycles Real and Imaginary: The Unexpectedly Long Old Age of Optical Lithography," *Research Policy* 24 (1995): 631–643.

13. Ian Ross, *Microelectronics: Progress and Prospects* [Sixth Mountbatten Lecture, November 10, 1983] (National Electronics Council, 1983).

14. Schattenburg, "History of the 'Three Beams.'"

15. Hyungsub Choi, Manufacturing Knowledge in Transit: Technical Practice, Organizational Change, and the Rise of the Semiconductor Industry in the United States and Japan, 1948–1960, PhD dissertation, Johns Hopkins University, 2007.

16. Jeffrey T. Macher, David C. Mowery, and David A. Hodges, "Reversal of Fortune? The Recovery of the US Semiconductor Industry," *California Management Review* 41 (1998): 107–136.

17. Scott Callon, *Divided Sun: MITI and the Break-Down of Japanese High-Tech Industrial Policy, 1975–1993* (Stanford University Press, 1995).

18. D. Kahng, "Trip Report—Japan (Toshiba, Matsushita, Fujitsu, and NEC) August 30–September 10, 1976," January 10, 1977, Bell Labs archives, collection 607–03–02, folder 2.

19. Richard N. Langlois and W. Edward Steinmuller, "Strategy and Circumstance: The Response of American Firms to Japanese Competition in Semiconductors, 1980–1995," December 1999 (http://papers.ssrn.com/sol3/papers.cfm?abstract_id=204093).

20. Laurence Berlowitz et al., "Instrumentation Needs of Research Universities," *Science* 211 (March 6, 1981): 1013–1018; An Assessment of the Needs for Equipment, Instrumentation, and Facilities for University Research in Science and Engineering (National Academy of Sciences, 1971).

21. Jay Harris, "It's a Small World," text of talk delivered at the 25th anniversary of the Cornell Nanofabrication Facility/NRRFSS in 2003, author's collection.

22. E. D. Wolf and J. M. Ballantyne, "Research and Resource at the National Submicron Facility," in *VLSI Electronics: Microstructure Science*, volume 1, ed. Norman G. Einspruch (Academic Press, 1981).

23. R. Guyford Stever, "Remarks," undated but probably 1977, in William O. Baker Papers, Box 40, Folder "PCST Energy—1977," Public Policy Papers, Department of Rare Books and Special Collections, Princeton University Library.

24. Harris, "It's a Small World."

25. Jay H. Harris et al., "The Government Role in VLSI," in *VLSI Electronics: Microstructure Science*, volume 1, ed. Einspruch. Also see comment by William Brinkman of Bell Labs in "Discussion of the Objectives, Program and Organization of the Proposed Center," in *Report of the NSF Workshop on Needs for a National Research and*

Resource Center in Submicron Structures (East Coast), submitted by J. N. Zemel and M. S. Chang (Philadelphia: Moore School of Electrical Engineering, University of Pennsylvania, May 10, 1976).

26. Harris, "The Government Role."

27. William S. C. Chang, Marcel W. Muller, Fred J. Rosenbaum, and Charles M. Wolfe, *Opportunities and Requirements for a National Center for Research on Submicron Structures* (Laboratory for Applied Electronic Sciences, Washington University, May 3, 1976), p. 5.

28. Zemel and Chang, *Report of the [Penn] Workshop*, p. 49–50. See also *Needs for a National Research and Resource Center in Submicron Structures: Report on National Science Foundation Workshop Held in Salt Lake City, Utah May 21, 1976*, submitted by Richard W. Grow, Robert J. Huber, and Roland W. Ure Jr. (Salt Lake City: Microwave Device and Physical Electronics Laboratory, University of Utah, 15 Sep, 1976), pp. 106–107.

29. Robert W. Keyes, Gordon Research Conference Proposal for 1976, received by GRC March 6, 1975, Records of the Gordon Research Conferences, Series III, Box 51, Folder Microstructure Fabrication, Chemical Heritage Foundation.

30. A. A. Daemmrich, N. R. Gray, and L. Shaper, eds., *Reflections from the Frontiers, Explorations for the Future: Gordon Research Conferences, 1931–2006* (Chemical Heritage Press, 2006).

31. H. M. Haendler, monitor report on Chemistry and Physics of Microstructure Fabrication conference, June 21–25, 1976, papers of the Gordon Research Conferences, Series VI, Box 128, Section Annual Evaluation Report 1976, Chemical Heritage Foundation.

32. Robert W. Keyes, letter to Alexander Cruickshank (director of the Gordon Research Conferences), July 14, 1976, papers of the Gordon Research Conferences, Series III, Box 51, Folder Microstructure Fabrication, Chemical Heritage Foundation. Also, Edward Wolf, testimony before the House Committee on Science and Technology, Subcommittee on Science, Research, and Technology hearing on Government and Innovation: University-Industry Relations, July 31, 1979, p. 135.

33. Henry Smith (PI), "Proposal submitted to National Science Foundation for National Research and Resource Facility for Submicron Structures," July 1, 1977, author's collection.

34. H. Smith, "X-Ray Lithography," in *Report of the NSF Workshop on Needs for a National Research and Resource Center in Submicron Structures (East Coast)*, submitted by J. N. Zemel and M. S. Chang (Philadelphia: Moore School of Electrical Engineering, University of Pennsylvania, May 10, 1976); Schattenburg, "History of the 'Three Beams;'" Henry I. Smith and R. Fabian Pease, "Reaching for the Bottom: The evolution of EIPBN," *Journal of Vacuum Science and Technology B* 22.6 (2004), 2882–2884.

35. “Some reasons why Cornell has a strong chance to attract such a center,” undated but probably summer 1976, probably from Joe Ballantyne and/or Charles Lee, in Cornell Center for Materials Research records, #53–24–3676, Division of Rare and Manuscript Collections, Cornell University Library, Box 29, Folder 37.

36. Cyrus C. M. Mody and Hyungsub Choi, “From Materials Science to Nanotechnology: Institutions, Communities, and Disciplines at Cornell University,” *Historical Studies in the Natural Sciences* 43.2 (2013): 121–161.

37. Cornell Center for Materials Research, “Some reasons why.”

38. Keyes proposal.

39. Joseph Ballantyne et al., proposal for National Sub-Micron Facility, undated [must be from January 1977], Cornell University College of Engineering Dean’s Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 39, Folder 14.

40. E. T. Cranch, Dean of the College of Engineering, memorandum for the record, re: “Telephone discussion with Professor T. Everhart, November 15, 1977,” November 16, 1977, in Cornell University College of Engineering Dean’s Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 37, Folder 33.

41. Cornell Center for Materials Research, “Some reasons why.”

42. Jay N. Zemel (PI), “Proposal for a National Center for Submicron Structure Research” [University of Pennsylvania/Drexel University/Lehigh University], January 1977, author’s collection.

43. Smith, MIT/Lincoln proposal. For Berkeley, see Christophe Lécuyer, “Semiconductor Innovation and Entrepreneurship at Three University of California Campuses,” in *Public Universities and Regional Growth: Insights from the University of California*, ed. Martin Kenney and David C. Mowery (Stanford University Press, 2014); Martin Kenney, David C. Mowery, and Donald Patton, “Electrical Engineering and Computer Science at UC Berkeley and in the Silicon Valley: Modes of Regional Engagement,” in *Public Universities and Regional Growth*, ed. Kenney and Mowery.

44. Smith oral history.

45. Ibid.

46. MIT Research Laboratory for Electronics, *Currents* 2.1 (1988): 10. Also see Smith oral history and *Report of the President and Chancellor 1977–78*, Massachusetts Institute of Technology.

47. Ibid. (all references). Also see *Report of the President and Chancellor 1978–79*, Massachusetts Institute of Technology.

48. Electrical Engineering Faculty Meeting minutes, October 3, 1977, forwarded to Dean E. Cranch, in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 37, Folder 33.

49. James Meindl, "The Center from the View of the User," in *Needs for a National Research and Resource Center in Submicron Structures: Report on National Science Foundation Workshop Held in Salt Lake City, Utah May 21, 1976*, submitted by Richard W. Grow, Robert J. Huber, and Roland W. Ure Jr. (Salt Lake City: Microwave Device and Physical Electronics Laboratory, University of Utah, September 15, 1976).

50. Forest Baskett et al. (including Linvill, Meindl, and James Gibbons), Proposal to Establish the Center for Integrated Systems at Stanford University, August 1979, CIS in-house archive.

51. "A New Home for the Facility," *PROSUS* (Program on Submicrometer Structures) newsletter, spring 1980, in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 32, Folder 19.

52. Hyungsub Choi has documented that Cornell received about $4 million from ARPA (about half the construction cost) to build Clark Hall for its Materials Science Center; see "The Physics-ARPA Building," August 22, 1960, Cornell Center for Materials Research records, #53–24–3676, Division of Rare and Manuscript Collections, Cornell University Library, Box 4, Folder 10. At Stanford, ARPA "agreed to provide 35,000 square feet of research space in a proposed 84,600-square-foot building" for materials science; program for dedication of Jack A. McCullough Building, October 19, 1965, Stanford University Archives, Collection 3258 McCullough Building. MIT's CMSE also used ARPA support to construct a new building; *President's Report Issue 1963–1964*, Massachusetts Institute of Technology.

53. "A New Home for the Facility."

54. Kevin Gross, "CIS Groundbreakers Laud Research-Industry Ties," *Stanford Daily*, May 20, 1983, in CIS in-house archive.

55. I have found in the Center for Integrated Systems in-house archive a dozen letters exchanged between members of the Stanford faculty (Linvill, Meindl, and Bill Spicer) and Harris, Krumhansl, and other NSF officers between June 27 and September 29, 1978. The CIS archive also contains a number of letters written in the same period that circulated either within Stanford or within federal science policy circles discussing plans for the CIS.

56. Joseph Ballantyne, "Introduction," in *Proceedings: NSF Workshop on Opportunities for Microstructures Science, Engineering and Technology in cooperation with the NRC Panel on Thin Film Microstructure Science and Technology: November 19–22, 1978* (National Science Foundation, 1978).

57. National Research Council panel on Thin-Film Microstructure Science and Technology, *Microstructure Science, Engineering, and Technology* (National Academy of Sciences, 1979), 1.

58. Ibid.

59. Ibid.

60. *Physics Today* can be thought of as a "para-scientific media" outlet of the sort Sarah Kaplan and Joanna Radin have described as disseminating (while modulating) scientific knowledge away from its sites of production; see Sarah Kaplan and Joanna Radin, "Bounding and Emerging Technology: Para-Scientific Media and the Drexler-Smalley Debate about Nanotechnology," *Social Studies of Science* 41 (2011): 457–485. Another para-scientific forum that spoke to Krumhansl's vision was *Mosaic*, the NSF's in-house magazine; see William Cromie, "Microfabrication: Room at the Bottom," *Mosaic* 12.3 (1981): 24–29.

61. W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and a Limitless Future* (Princeton University Press, 2013); Chris Toumey, "Reading Feynman into Nanotechnology: A Text for a New Science," *Techné* 12.3 (2008): 133–168.

62. For a grant proposal that referenced the Krumhansl-Pao article, by a scientist who later figured prominently in nanotechnology, see Calvin Quate, "Research on Acoustic Microscopy with Superior Resolution," proposal to NSF, 1981, C. F. Quate Papers (SC0347), Department of Special Collections and University Archives, Stanford University Libraries, Box 3, Folder "NSF 10786 Proposal December 1981."

63. John F. Mason, "VLSI Goes to School," *IEEE Spectrum*, November 1980: 48–52.

64. Jerry R. Yeargan, "Developing a Program in Analog Electronics," in *Proceedings of the Sixth Biennial University/Government/Industry Microelectronics Symposium* (IEEE, 1985); Robert M. Burger, *Cooperative Research: The New Paradigm* (Semiconductor Research Corporation, 2001). Note that the SRC operates largely through project grants to universities, and that its first University Advisory Committee was composed of Ed Wolf, John Linvill, Ben Streetman from the University of Illinois (who would soon found Texas' MRC), Paul Penfield (associated with the MIT Submicrometer Structures Laboratory), faculty members associated with the Arizona State, Caltech, and Minnesota facilities listed above, and professors from Berkeley and Carnegie-Mellon.

65. R. C. Jaeger et al., "The Alabama Microelectronics Science and Technology Center and the Microelectronics Program at Auburn University," in *Proceedings of the Sixth Biennial University/Government/Industry Microelectronics Symposium* (IEEE, 1985).

66. Ben Streetman et al., "Proposal for a Texas Microelectronics Center in the College of Engineering, The University of Texas at Austin," March 1983, in University of Texas Executive Vice President and Provost's Office records (collection 96–273),

Dolph Briscoe Center for American History, University of Texas at Austin, Box 24, Folder Microelectronics Research Center, 1983–1984.

67. Letter from Prof. Peter A. Wolff to Prof. J. Allen, August 23, 1977, re: JSEP Topical Review of Semi-conductor Integrated Circuits, Devices, and Materials, Stanford, 3, 4 August 1977, Jerome Wiesner Administration Records (AC 8), Institute Archives and Special Collections, MIT Libraries, Box 205, Folder Research Laboratory for Electronics.

68. Joe Ballantyne, memo to Greg Galvin, re: Information for Frank Rhodes [president of Cornell], August 11, 1986, Frank H. T. Rhodes papers, #3–12–1795, Division of Rare and Manuscript Collections, Cornell University Library, Box 160, Folder 59.

69. NRRFSS Policy Board, "Background material for topics to be discussed," 17 Nov 1986, in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 39, Folder 13.

70. Streetman, "Proposal," appendix reporting on talks by microfabrication facility directors from (in order or presentation) Stanford, Caltech, Cornell, RPI, Minnesota, North Carolina, Arizona State, Berkeley, and MIT.

71. I found a description of such a phone call in a letter from John Young (of Hewlett-Packard), to Bruce Hinchcliffe, dated July 14, 1981, in the Stanford Center for Integrated Systems in-house archive.

72. MIT Microsystems Industrial Group Prospectus, May 1989, found in Stanford CIS in-house archive.

73. MIT patent policy draft of May 1982, with handwritten note indicating that it came "from SRC [Semiconductor Research Corporation] file" of John Linvill, director of Stanford Center for Integrated Systems. The patent policy was attached to a memo from Jim Gibbons to CIS Executive Committee, October 29, 1982, found in the Stanford Center for Integrated Systems in-house archive.

74. NRRFSS, "Report on the National User Research Program '82–'85," in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 38, Folder 5; E. D. Wolf, "Recent National Press Coverage of NRRFSS," handout for NSF discussions, 7 Jan 1982, Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 37, Folder 32.

75. Richard Atkinson, testimony before the House Committee on Science and Technology, Subcommittee on Science, Research, and Technology hearing on Government and Innovation: University-Industry Relations, July 31, 1979, p. 82.

76. Frank H. T. Rhodes, testimony before the House Committee on Science and Technology, hearing on Improving the Research Infrastructure at US Universities and Colleges, May 8, 1984, p. 88.

77. T. E. Everhart, memo to NRRFSS Policy Board, February 25, 1981, in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 39, Folder 12.

78. For a review and reminiscence of Crewe's achievements, see Michael S. Isaacson, "Seeing Single Atoms," *Ultramicroscopy* 123 (2012): 3–12.

79. See M. Isaacson and A. Murray, "*In situ* Vaporization of Very Low Molecular Weight Resists Using ½ nm Diameter Electron Beams," *Journal of Vacuum Science and Technology* 19 (1981): 1117–1120.

80. Newman and Pease's letter to Feynman, his reply awarding them the $1,000, and his apology to Isaacson for earlier telling him "there was no more prize" (and therefore making Isaacson think there was no point in writing and submitting a full page of text before Pease did) is reprinted on pp. 391–393 of Richard P. Feynman, *Perfectly Reasonable Deviations from the Beaten Track: The Letters of Richard P. Feynman*, ed. Michelle Feynman (Basic Books, 2008). The *Tale of Two Cities* micrograph appears in T. H. Newman, K. E. Williams, and R. F. W. Pease, "High Resolution Patterning System with a Single Bore Objective Lens," *Journal of Vacuum Science and Technology B* 5 (1987): 88–91.

81. See, for instance, Henry I. Smith and M. L. Schattenburg, "X-Ray Lithography from 500 nm to 30 nm: X-Ray Nanolithography," *IBM Journal of Research and Development* 37 (1993): 319–329.

82. Smith oral history.

83. Edward Wolf, "A Personalized Summary of CNF History," Future of Nanotechnology symposium (thirtieth anniversary of Cornell NanoScale Science and Technology Facility, June 14, 2007).

84. Ronald Gutman [NSF Program Director for Solid State and Microstructures Engineering] to Ed Wolf, July 1982, and notification letter from NSF grants office to Cornell Office of Sponsored Research, July 19, 1982, both in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 39, Folder 15.

85. Report on the National User Research Program.

86. E. D. Wolf et al., "Proposal for National Nanofabrication Facility at Cornell University," Aug 1986, Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 39, Folder 15. It should be noted, however, that the next sentence of the proposal states that "the resources of the NRRFSS are available 24 hours a day, 365 days a year to trained users." It is likely, therefore, that the 60% figure overstates the proportion of non-Cornell users. Non-Cornell visitors would depend on assistance from facility personnel (who were more likely to be present during normal working hours) for a much greater proportion of their time in Ithaca than Cornell

users. Thus, Cornell graduate students would probably have found it easier to use the facility's equipment during off-hours, and therefore probably took up more than 40% of total user time.

87. Report on the National User Research Program.

88. Ballantyne to Galvin, August 11, 1986.

89. Letter from the corporate members of the NRRFSS Policy Board (from Intel, IBM, Motorola, Bell Labs, and GE) to Nam Suh, NSF Assistant Director for Engineering, October 16, 1986, Frank H. T. Rhodes papers, #3–12–1795, Division of Rare and Manuscript Collections, Cornell University Library, Box 160, Folder 59.

90. Memo from Edward D. Wolf to President Frank Rhodes, March 17, 1986, re: Appreciation for your efforts on the behalf of NRRFSS and comments regarding Mr. Bloch's response to your recent telephone call, Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 38, Folder National Research and Resource Facility for Submicron Structures.

91. Remarks of Mr. Erich Bloch, Director NSF, at the dedication of the center building, Center for Integrated Systems, Stanford University, May 23, 1985, National Archives and Records Administration II, National Science Foundation, NSF Historian collection, 307–130–37–16-(1–6), Box 47, Folder 5–23–85 Stanford Univ.

92. Belanger, *Enabling American Innovation*, p. 219.

93. Submicron Facility Policy Board letter, October 16, 1986.

94. Nam Suh, NSF Assistant Director for Engineering, to National Science Board Committee on Programs and Plans, around October 16, 1986, re: Report on User Research at the National Research and Resource Facility for Submicron Structures, in Frank H. T. Rhodes papers, #3–12–1795, Division of Rare and Manuscript Collections, Cornell University Library, Box 160, Folder 59.

95. Joseph M. Ballantyne, "Centers: Collaboration, Coordination, Competition, Collegiality, Cost, and Continuity," draft for talk at symposium in honor of Dale Corson, author's collection.

96. "The Role of NRRFSS" ["a report prepared for Cornell University President Frank H. T. Rhodes describing the past accomplishments of NRRFSS"], in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 39, Folder 16.

97. Edward D. Wolf (PI), proposal for Cornell Nanoelectronics Engineering Center, August 30, 1985, in Frank H. T. Rhodes papers, #3–12–1795, Division of Rare and Manuscript Collections, Cornell University Library, Box 139, Folder 56 National Science Foundation.

98. Cyrus C. M. Mody, *Instrumental Community: Probe Microscopy and the Path to Nanotechnology* (MIT Press, 2011), chapter 6.

Chapter 5

1. "Report of the White House Science Council Panel on Semiconductors," September 1987, William Graham Files, Ronald Reagan Presidential Library, Box CFOA 990, Folder "Semiconductors (1)."

2. Charles E. Sporck [National Semiconductor CEO], The Japanese Threat: Courteous Destruction (Santa Clara: National Semiconductor, presumably 1978 [from talk delivered in 1978]), found in Box 38, Folder "The Japanese Threat: Courteous Destruction," Richard Thomas deLamarter collection of IBM antitrust suit records (accession 1980), Hagley Museum and Library.

3. On semiconductor/microelectronics research consortia, see among others Larry D. Browning and Judy C. Shetler, *Sematech: Saving the US Semiconductor Industry* (Texas A&M University Press, 2000); David V. Gibson and Everett M. Rogers, *R&D Collaboration on Trial: The Microelectronics and Computer Technology Corporation* (Harvard Business Review Press, 1994); E. Raymond Corey, *Technology Fountainheads: The Management Challenge of R&D Consortia* (Harvard Business School Press, 1997); W. J. Spencer and P. Grindley, "SEMATECH after Five Years: High-Technology Consortia and US Competitiveness," *California Management Review* 35 (1993): 9–32; Robert M. Burger, *Cooperative Research: The New Paradigm* (Semiconductor Research Corporation, 1996). On contemporaneous industry-oriented academic microelectronics centers, see Elizabeth Popp Berman, *Creating the Market University: How Academic Science Became an Economic Engine* (Princeton University Press, 2012); Stuart W. Leslie, "Regional Disadvantage: Replicating Silicon Valley in New York's Capital Region," *Technology & Culture* 42.2 (2001): 236–264; Christophe Lécuyer, "What Do Universities Really Owe Industry? The Case of Solid State Electronics at Stanford," *Minerva* 43 (2005): 51–71; Irwin Feller, Catherine P. Ailes, and J. David Roessner, "Impacts of Research Universities on Technological Innovation in Industry: Evidence from Engineering Research Centers," *Research Policy* 31 (2002): 457–474; B. Bozeman and B. Boardman, "The NSF Engineering Research Centers and the University-Industry Research Revolution: A Brief History Featuring an Interview with Erich Bloch," *Journal of Technology Transfer* 29 (2004): 365–375.

4. Jeffrey T. Macher, David C. Mowery, and David A. Hodges, "Reversal of Fortune? The Recovery of the U.S. Semiconductor Industry," *California Management Review* 41 (1998): 107–136.

5. David P. Angel, *Restructuring for Innovation: The Remaking of the US Semiconductor Industry* (Guilford, 1994).

6. SEMATECH Strategic Overview, December 1991, Texas Instruments Records ACCN 94–25, DeGolyer Library, Southern Methodist University, Box/Folder SEMATECH Strategic Overview and Accomplishments.

7. AnnaLee Saxenian, "Contrasting Patterns of Business Organization in Silicon Valley," *Environment and Planning D: Society and Space* 10 (1992): 377–391.

8. W. W. Powell, K. W. Koput, and L. Smith-Doerr, "Interorganizational Collaboration and the Locus of Innovation: Networks of Learning in Biotechnology," *Administrative Science Quarterly* 41 (1996): 116–145; J. Hagedoorn, "Inter-firm R&D Partnerships: An Overview of Major Trends and Patterns since 1960," *Research Policy* 31 (2002): 477–492.

9. Philip Mirowski explores the effects of the breakdown of the Chandlerian firm on American science at length in *Science-Mart: Privatizing American Science* (Harvard University Press, 2011).

10. Michael Riordan and Lillian Hoddeson, *Crystal Fire: The Invention of the Transistor and the Birth of the Information Age* (Norton, 1997); Hyungsub Choi, Manufacturing Knowledge in Transit: Technical Practice, Organizational Change, and the Rise of the Semiconductor Industry in the United States and Japan, 1948–1960, PhD dissertation, Johns Hopkins University, 2007.

11. Glenn Zorpette, "Electronics Consortia to Impact Products for Generations," *IEEE Spectrum*, October 1990: 50–52.

12. Report of the White House Science Council Panel on Semiconductors.

13. Gibson and Rogers, *R&D Collaboration on Trial*.

14. Economic Policy Council R&D Working Group, draft "University Technology Research," December 5, 1985, George Keyworth Files, RAC Box 4, folder "Economic Policy Council (R&D Working Group)—1985" (2 of 3), Ronald Reagan Presidential Library. Underlining in original.

15. Joel N. Shurkin, *Broken Genius: The Rise and Fall of William Shockley, Creator of the Electronic Age* (Macmillan, 2006).

16. David C. Brock, *Understanding Moore's Law: Four Decades of Innovation* (Chemical Heritage Foundation, 2006).

17. Carver Mead, "Silicon Structures Project," speech given in Palm Springs, February 12, 1980, Caltech Archives.

18. Ibid.

19. Ibid.

20. Ibid.

21. Berman, *Creating the Market University*.

22. Lécuyer, "What Do Universities Really Owe Industry?" Also see Lécuyer, *Making Silicon Valley: Innovation and the Growth of High Tech, 1930–1970* (MIT Press, 2006); Leslie, "Regional Disadvantage"; Stuart W. Leslie, *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (Columbia University Press, 1993).

23. "Background: Visiting Scientists, Industrial Affiliates Participate in National Research and Resource Facility for Submicron Structures," Cornell University News Bureau news release October 1981, in Cornell University College of Engineering Dean's Office records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 37, Folder 32.

24. Semiconductor Research Corporation, 1983 Annual Report (https://www.src.org/about/corporate-annual/1983.pdf).

25. H. Craig Casey, "The Microelectronics Center of North Carolina: A New Program in Science and Integrated Circuits," in *Proceedings of the Fourth Biennial University/Government/Industry Microelectronics Symposium* (IEEE, 1981); Dale Whittington, *High Hopes for High Tech: Microelectronics Policy in North Carolina* (University of North Carolina Press, 1985).

26. University of Texas Office of Public Affairs, news release for July 12, 1984, in UT Office of Public Affairs Records, Dolph Briscoe Center for American History, University of Texas at Austin, Subject Files, Box 4Ac86, Folder "Microelectronics Research Center."

27. University of Texas Office of Public Affairs, news release for April 9, 1987, in UT Office of Public Affairs Records, Dolph Briscoe Center for American History, University of Texas at Austin, Subject Files, Box 4Ac86, Folder "Microelectronics Research Center."

28. Leslie, "Regional Disadvantage."

29. SEMATECH and Newly Merged SUNY CNSE/SUNYIT Partner in Launch of Innovative Planarization Center," PRWEB, July 8, 2014 (http://www.prweb.com/releases/2014/07/prweb11996562.htm).

30. Viola Vogel, Our Vision for the Center for Nanotechnology, draft of September 19, 1996, author's collection.

31. Whittington, *High Hopes for High Tech*, p. 138. Also see Gregory T. Cibuzar, "Microelectronics at the University of Minnesota," in *Proceedings of the Tenth Biennial University/Government/Industry Microelectronics Symposium* (IEEE: 1993).

32. Letter from Ray Warner to Jack Kilby, October 17, 1983, Jack S. Kilby Papers, Manuscript Division, Library of Congress, Box 11, Folder 16 Correspondence 1983, Oct.

33. John G. Linvill, "Progress Report on the Stanford Center for Integrated Systems," January 1982, CIS in-house archive, SFR index folder.

34. Remarks of Mr. Erich Bloch, Director NSF, at the dedication of the center building, Center for Integrated Systems, Stanford University, May 23, 1985, National Archives and Records Administration II, National Science Foundation, NSF Historian collection, 307–130–37–16-(1–6), Box 47, Folder 5–23–85 Stanford Univ.

35. SRC, 1983 Annual Report.

36. "CIS Building Pledge Program," memo dated 11/82, CIS in-house archive, SFR index folder.

37. Joe Vollmer, "Les Hogan of Fairchild Discusses the Semiconductor Industry," *CIS Newsletter*, September 1982: 1–2.

38. Ibid.

39. Evelyn Richards, "Stanford Moves Full Speed Ahead on Chip Research," *San Jose Mercury*, December 28, 1981.

40. Mary Duenwald, "Center for Integrated Systems: Will It Design Our Hi-Tech Future?" *Palo Alto Weekly* III.21 (1982): 9–12.

41. David C. Mowery, "*Plus ça change*: Industrial R&D in the 'Third Industrial Revolution,'" *Industrial and Corporate Change* 18 (2009): 1–50.

42. Economic Policy Council R&D Working Group, draft "University Technology Research," December 5, 1985, George Keyworth Files, RAC Box 4, folder 2 (of 3) titled "Economic Policy Council (R&D Working Group) - 1985," Ronald Reagan Presidential Library.

43. Robert R. Schaller, Technological Innovation in the Semiconductor Industry: A Case Study of the International Technology Roadmap for Semiconductors (ITRS), PhD dissertation, George Mason University, 2004.

44. "Silicon Valley, Stanford's Baby, Has International Challenges," *Stanford Observer*, April 1981: 1, 5, 7.

45. "TSMC Joins CIS," *CIS Newsletter*, winter 2001.

46. Interview with James Plummer conducted by the author, August 11, 2010, Palo Alto.

47. CIS Industrial Sponsors, Advisory Committee, draft of Sub-Committee Report, September 16, 1988, CIS in-house archive, CIS executive committee binder, 1988–1989.

48. Ibid.

49. Letter from John Linvill to Gordon Moore, March 28, 1990, CIS in-house archive, sponsor advisory committee folder.

50. Data in this paragraph are from American Association for the Advancement of Science, “Historical Trends in Federal R&D” (http://www.aaas.org/page/historical-trends-federal-rd).

51. Source of first quotation: Edward D. Wolf, “A Comment from the Director,” *PROSUS* [Program on Submicrometer Structures] newsletter, spring, 1980. Source of second quotation: “Using the Submicron Facility: A Unique Opportunity for Researchers throughout the United States,” in *Submicron Research: Investigations at Cornell University in the National Submicron Facility*, ed. Hillary Rettig (Cornell College of Engineering, 1984).

52. Robert Gannon, “How Science Probes the Weird World of the Ultrasmall,” *Popular Science*, June 1982: 62–64; Sharon Begley with John Carey, “Exploring the Microworld,” *Newsweek*, October 26, 1981, p. 85.

53. Sidebar on “Who Are the Users of the NNF?” in Edward D. Wolf, “Ten Years Old and Going Strong: A Commemoration of a Decade of Research at Cornell’s National Nanofabrication Facility,” *Engineering: Cornell Quarterly* 22.3 (1988): 2–7.

54. Harvey C. Hoch and Richard C. Staples, “How a Fungus Recognized Surface Topography: A Problem Solved through Microfabrication,” *Engineering: Cornell Quarterly* 22.3 (1988): 30–34.

55. John C. Sanford, “The Development of the Biolistic Process,” *In Vitro Cellular and Developmental Biology—Plant* 36.5 (2000): 303–308.

56. Ibid.

57. Ibid.

58. Nicole Nelson, “Shooting Genes, Distributing Credit: Narrating the Development of the Biolistic Gene Gun,” *Science as Culture* 21.2 (2012): 205–232.

59. H. G. Craighead, 1993–94 Annual Report for National Nanofabrication Facility, Cornell University College of Engineering Dean’s Office Records, #16–2–2156, Division of Rare and Manuscript Collections, Cornell University Library, Box 61, Folder 8 (National Nanofabrication Facility).

60. Lynn W. Jelinski, Harvey C. Hoch, and Harold G. Craighead, “Introduction,” in *Nanofabrication and Biosystems: Integrating Materials Science, Engineering, and Biology*, ed. Hoch, Jelinski, and Craighead (Cambridge University Press, 1996).

61. Linvill, “Progress Report on the Stanford Center for Integrated Systems.”

62. Research Activities of the CIS Affiliated Faculty (CIS, 1985).

63. Ibid. One other project, with James Angell as PI, was also a relic of 1970s collaborations with music: “Digital Systems to Enhance Auditorium Acoustics.” In addition, the very last abstract in the 1985 CIS research report describes “the WORLD AFFAIRS REPORT … the only journal … the complete text of which, from

its first issues in 1970, is online. It is incidentally the only online journal produced at Stanford"—though, in fact, the Report was edited by a *former* Stanford professor of Romance languages, Ronald Hilton. It isn't clear what Hilton's tie to the CIS was, though his abstract implies the possibility of transplanting his on-line journal model to the physical and engineering sciences.

64. "Gibbons Sees Wide Impact for Microelectronics," Stanford University News Service press release, March 5, 1984, CIS in-house archive, publicity folder.

65. Kevin Gross, "Construction Set for New Research Center," *Stanford Daily* (May 13, 1983).

66. Tim Lenoir and Eric Giannella, "The Emergence and Diffusion of DNA Microarray Technology," *Journal of Biomedical Discovery and Collaboration* 1.11 (2006).

67. Doogab Yi, *The Integrated Circuit for Bioinformatics: The DNA Chip and Material Innovation at Affymetrix* (Chemical Heritage Foundation, 2010).

68. Rebecca Henderson, "Of Life Cycles Real and Imaginary: The Unexpectedly Long Old Age of Optical Lithography," *Research Policy* 24 (1995): 631–643.

69. Lenoir and Giannella, "Emergence and Diffusion."

70. Stephen M. Edgington, "Biotech's New Nanotools," *Bio/Technology* 12 (May 1994): 468–471.

71. Cyrus C. M. Mody, "Conferences and the Emergence of Nanoscience," in *The Social Life of Nanotechnology*, ed. Barbara Herr Harthorn and John Mohr (Routledge, 2012).

72. "Written Comments Made by Conferees 1988," Series VI (Evaluations), Box 129, Folder 4 (1983–1989), Gordon Research Conference papers, Chemical Heritage Foundation.

73. Conference agenda, Series V (Programs), Box 124b, Folder 11 (1994), Gordon Research Conference papers, Chemical Heritage Foundation.

74. J.D. Plummer, report on "Building Chips in a University Environment—the Stanford BICMOS Project," October 1988, in CIS in-house archive, CIS Executive Committee binder, 1988–1989.

75. Nanofabrication Users Workshop, *Nanofabrication Workshop Report* (report from September 16–17, 1991 workshop). Notably, the Directorate for Biological, Behavioral, and Social Sciences was one of three NSF directorates funding the workshop.

76. National Science Foundation Directorate for Engineering Division of Electrical and Communications Systems Solid State and Microstructures Program, "National Nanofabrication Users Facility Proposal Solicitation" (late 1991).

77. James D. Plummer (PI), proposal for National Nanofabrication Users Facility, March 9, 1992, author's collection.

78. Memo with draft NNUF proposal from Jim Plummer to NSF Proposal Team, February 18, 1992, author's collection.

79. Site-visit review for National Nanofabrication Network competition, July 20, 1993, author's collection.

80. Ibid.

81. Lynn Simarski, "National Science Board Approves Creation of National Nanofabrication Users Network," NSF press release 93–88, November 22, 1993.

82. Quotation from the NNUN solicitation found in Lawrence Goldberg, "Reflecting on 30 Years of NSF Investment in Cornell's National User Facility Leading to the National Nanotechnology Infrastructure Network," Future of Nanotechnology symposium (30th anniversary of Cornell NanoScale Science and Technology Facility, June 14, 2007).

83. Jason Gallo, "The Discursive and Operational Foundations of the National Nanotechnology Initiative in the History of the National Science Foundation," *Perspectives on Science* 17.2 (2009): 174–211.

84. Joel M. Podolny and Karen L. Page, "Network Forms of Organization," *Annual Review of Sociology* 24 (1998): 57–76.

85. Ibid.

86. Daniel J. Kevles, "Big Science and Big Politics in the United States: Reflections on the Death of the SSC and the Life of the Human Genome Project," *Historical Studies in the Physical and Biological Sciences* 27 (1997): 269–297.

87. John F. Burness, Vice President for University Relations, to Frank Rhodes, President of Cornell, September 10, 1986, in Frank H. T. Rhodes papers, #3-12-1795, Division of Rare and Manuscript Collections, Cornell University Library, Box 160, Folder 59.

88. Peter J. Westwick, *The National Labs: Science in an American System, 1947–1974* (Harvard University Press, 2003).

89. Roger Segelken, "A Case History of a Computer Media Event—Introducing a Supercomputer Center," in *Proceedings of the Fourth International Conference on Systems Documentation* (ACM, 1985).

Chapter 6

1. Arthur L. Robinson, "Nanocomputers from Organic Molecules?" *Science* 220 (1983): 940–942.

2. Allen A. Boraiko, “The Chip: Electronic Mini-Marvel That Is Changing Your Life,” *National Geographic* 162.4 (1982): 421–458 (ellipses in original).

3. Cyrus C. M. Mody, *Instrumental Community: Probe Microscopy and the Path to Nanotechnology* (MIT Press, 2011).

4. A. Aviram, C. Joachim, and M. Pomerantz, “Evidence of Switching and Rectification by a Single Molecule Effected with a Scanning Tunneling Microscope,” *Chemical Physics Letters* 146.6 (1988): 490–495.

5. Ari Aviram, “Molecular Electronics—Science and Technology,” *Advanced Materials* 1.4 (1989): 124–125.

6. Robert W. Keyes, “Minimum Size Devices,” in *Molecular Electronics—Science and Technology*, ed. Ari Aviram (Engineering Foundation, 1989).

7. This section is based on interviews with anonymous participants, but also broadly parallels the narrative in Rick Overton, “Molecular Electronics Will Change Everything,” *Wired* 8.07 (July 2000).

8. Overton, “Molecular Electronics.”

9. For an overview, see Andrew Schrank and Josh Whitford, “The Anatomy of Network Failure,” *Sociological Theory* 29 (2011): 151–177. The “screwed up” / “screwed us” dichotomy is from Philipp Brandt and Josh Whitford, “Tools to Build Ties,” paper presented at Industry Studies Conference, Kansas City, May 30, 2013.

10. Source: anonymous interview. For an examination of DARPA’s embrace of the network approach, see Erica R. H. Fuchs, “Rethinking the Role of the State in Technology Development: DARPA and the Case for Embedded Network Governance,” *Research Policy* 39 (2010): 1133–1147.

11. Kwan S. Kwok and James C. Ellenbogen, “Moletronics: Future Electronics,” *Materials Today* 5.2 (2002): 28–37. In describing the DARPA program, Kwok and Ellenbogen cite the Yale-Rice-PSU, HP-UCLA, Harvard, and Northwestern groups described here, and also cite a group at MITRE that was associated with Ellenbogen. They also mention a few affiliated teams, including Mayer and Mallouk at Penn State and Josef Michl’s group at the University of Colorado (which worked with Ratner, IBM, and others).

12. Ibid.

13. Robert M. Metzger, “Unimolecular Rectifiers and What Lies Ahead,” *Colloids and Surfaces A: Physicochemical and Engineering Aspects* 284–285 (2006): 2–10.

14. C. P. Collier, E. W. Wong, M. Belohradsky, F. M. Raymo, J. F. Stoddart, P. J. Kuekes, R. S. Williams, and J. R. Heath, “Electronically Configurable Molecular-Based Logic Gates,” *Science* 285 (1999): 391–394.

15. John Markoff, "A Renaissance in Computer Science; Chip Designers Search for Life after Silicon," *New York Times*, July 19, 1999.

16. Robert F. Service, "Next-Generation Technology Hits an Early Midlife Crisis," *Science* 302 (2003): 556–559.

17. James R. Heath, "Molecular Electronics," *Annual Review of Materials Research* 39 (2009): 1–23. The parenthetical remark was bracketed in the original; I have changed the brackets to parentheses here.

18. Ibid.

19. M. A. Reed, C. Zhou, C. J. Muller, T. P. Burgin, and J. M. Tour, "Conductance of a Molecular Junction," *Science* 278 (1999): 252–254.

20. Chris Gintz (CEO), Molecular Electronics Corporation Business Plan, July 15, 2002, in President's Office Records: President Malcolm Gillis, 1993–2004 (UA 100), Woodson Research Center, Fondren Library, Rice University, Box 154, Folder 6.

21. James M. Tour, Mark A. Reed, Jorge M. Seminario, David L. Allara, and Paul S. Weiss, "Molecular Computer," US Patent 6,430,511, issued August 6, 2002, filed January 20, 2000.

22. David Pacchioli, "Machining with Molecules," *Penn State News*, September 1, 2001.

23. Edwin A. Chandross, "More on Molecular Electronics," *Science* 303 (2004): 1137.

24. Here is a small sampling of that literature: Sarah Kaplan and Joanna Radin, "Bounding an Emerging Technology: Para-Scientific Media and the Drexler-Smalley Debate about Nanotechnology," *Social Studies of Science* 41.4 (2011): 457–485; Lee-Anne Broadhead and Sean Howard, "'Two Cultures,' One Frontier: The Drexler-Smalley Debate on the Limits and Potential of Nanotechnology," *Techné* 15.1 (2011): 23–35; Matthew N. Eisler, "'The Ennobling Unity of Science and Technology': Materials Science and Engineering, the Department of Energy, and the Nanotechnology Enigma," *Minerva* 51 (2013): 225–251; W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and a Limitless Future* (Princeton University Press, 2013); Robert Sparrow, "Revolutionary and Familiar, Inevitable and Precarious: Rhetorical Contradictions in Enthusiasm for Nanotechnology," *NanoEthics* 1 (2007): 57–68; Otávio Bueno, "The Drexler-Smalley Debate on Nanotechnology: Incommensurability at Work?" in *Nanotechnology Challenges*, ed. Joachim Schummer and Davis Baird (World Scientific, 2006).

25. Hugh Aldersey-Williams, *The Most Beautiful Molecule* (Aurum, 1995); Jim Baggott, *Perfect Symmetry: The Accidental Discovery of Buckminsterfullerene* (Oxford University Press, 1994); Philip Ball, *Designing the Molecular World* (Princeton University Press, 1994).

26. Ronald N. Kostoff, Raymond G. Koytcheff, and Clifford G. Y. Lau, "Technical Structure of the Global Nanoscience and Nanotechnology Literature," *Journal of Nanoparticle Research* 9 (2007): 701–724.

27. W. Patrick McCray, "Will Small Be Beautiful? Making Policies for Our Nanotech Future," *History and Technology* 21 (2005): 177–203.

28. Rice Quantum Institute, Annual Report of FY 1994–95, in Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 24, Folder 6.

29. Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (Princeton University Press, 1986), p. 231ff.

30. S. C. O'Brien, Y. Liu, Q. Zhang, J. R. Heath, F. K. Tittel, R. F. Curl, and R. E. Smalley, "Supersonic Cluster Beams of III-V Semiconductors: GaxAsy," *Journal of Chemical Physics* 84 (1986): 4074–4079. TI's gift of semiconductor discs is mentioned here and in Y. Liu, Q.-L. Zhang, F. K. Tittel, R. F. Curl, and R. E. Smalley, "Photodetachment and Photofragmentation Studies of Semiconductor Cluster Anions," *Journal of Chemical Physics* 85 (1986): 7434–7441.

31. Michael Berryhill and Greg Kahn, "Timing a Change," *Sallyport*, December—January 1992–1993: 22–26.

32. Ken Smith, RQI Enhancement Proposal—Preface and Executive Summary, in Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 24, Folder 4.

33. Memo from Ken Smith to RQI Fellows, February 16, 1987, Re: Summary of RQI Meeting: Thursday, February 5, in Neal Lane papers, 1964–2011, MS 502, Woodson Research Center, Fondren Library, Rice University, Box 8, Folder 1.

34. Smith, RQI Enhancement Proposal.

35. Ken Smith, memo to RQI Fellows, Re: Proposal for Research Center, March 4, 1987, Neal Lane papers, 1964–2011, MS 502, Woodson Research Center, Fondren Library, Rice University, Box 8, Folder 1.

36. Ken Smith, notes from RQI Research Center Meeting #1, March 8, 1987, Neal Lane papers, 1964–2011, MS 502, Woodson Research Center, Fondren Library, Rice University, Box 8, Folder 1.

37. Oral-history interview with Robert Curl Jr. conducted by the author, Houston, May 29, 2008.

38. Poster for RQI Informal Seminar/Discussion Series talk by Mike Alford, "Silicon Is Never Boring: Some New Results of Silicon Cluster Ion Reactivity," September 22, 1989, Rice Quantum Institute information file, Woodson Research Center, Fondren Library, Rice University.

39. R. E. Smalley, "C_{60}, Chapter 2," presented at meeting of American Physical Society, Cincinnati, 1991.

40. Melissa Williams, "President in Full Swing after 1st Months in Office," *Rice Thresher*, September 17, 1993 (brackets in original).

41. Michael Cinelli, "Issues and Answers/Looking Back on an Inaugural Year," *Rice News*, June 30, 1994, p. 8.

42. Memo from Malcolm Gillis (president of Rice) to Charles Duncan, Evans Attwell, and Kent Anderson (members of the board of trustees), August 18, 1992, Re: Nanotechnology: Standing in Awards of Prestigious Fellowships, Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 21, Folder 5. "There is very wide agreement that Rice should try very hard to keep Rick Smalley here, in the face of strong overtures from Berkeley and Princeton."

43. Curl interview.

44. Draft of Proposal for the Lillian Illig Post-Doctoral Chair in Nanotechnology, January 23, 1997, Richard E. Smalley papers, Chemical Heritage Foundation Collections, Box 56, Folder 6.

45. Letter from Radford Byerly Jr. (chief of staff, U.S. House of Representatives, Committee on Science, Space, and Technology) to Rick Smalley, 1 Mar. 1993, Richard Smalley Papers, 1990–1998, MS#490, Woodson Research Center, Rice University, Box 2, Folder 49.

46. Letter from Smalley to Michael Carroll (dean of engineering) and James Kinsey (dean of natural sciences), January 21, 1993, Re: Inter Divisional Committee on Nanotechnology, Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 21, Folder 6.

47. Colin Milburn, *Nanovision: Engineering the Future* (Duke University Press, 2008), p. 199.

48. Letter from Smalley to Carroll and Kinsey, January 21, 1993.

49. Todd Ackerman, "New Building to Keep Rice Up with Science and Prof on Campus," *Houston Chronicle*, November 11, 1993.

50. Michael Cinelli and Lia Unrau, "Rice's Interdisciplinary Science Centers: A Reporter's Resource," November 5, 1996, Lia Unrau science news research files, 1992–2001, MS 072, Woodson Research Center, Fondren Library, Rice University, Box 1, Folder 17.

51. Eisler, "Ennobling Unity."

52. Tom Siegfried, “Paying a Price: New Chief of Science Funding Agency Expects Projects to Face Increasing Scrutiny over Costs,” *Dallas Morning News*, December 20, 1993.

53. Mody, *Instrumental Community*, chapter 6; Martina Merz, “Reinventing a Laboratory: Nanotechnology as a Resource for Organizational Change,” in *Sociology of the Sciences Yearbook* 27 (2010): 3–19; Christian Kehrt, “From Do-It-Yourself Quantum Mechanics to Nanotechnology? The History of Semiconductor Physics, 1970–2000,” in *Traditions and Transformations in the History of Quantum Physics*, ed. Jürgen Renn, Robert Schlögl, and Bernard F. Schutz (Edition Open Access, 2013)

54. Report of the Nanotechnology Task Force, November 22, 1993, Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 21, Folder 7.

55. Richard E. Smalley, Proposal for Center for Carbon Nanotechnology, September 1998, in Richard E. Smalley papers, Chemical Heritage Foundation Collections, Box 31, Folder 5 National Science Foundation Science and Technology Center Proposal.

56. Reed, Lieber, and Gimzewski are mentioned in “Table 1: CNST Faculty Hiring,” undated but circa November 1997, in Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 21, Folder 4; Avouris is discussed in memo from Rick Smalley and Ken Smith to Malcolm Gillis (president) and David Auston (provost), September 13, 1996, Re: Annual Report for CNST, Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 21, Folder 3. Tour’s move is announced in Lia Unruh, “Leader in Molecular Electronics Joins Rice,” *Rice News*, October 7, 1999.

57. Email from Richard E. Smalley to John Pazik (ONR), September 2, 1997, Re: workshop and tubes@rice, Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 22, Folder 1.

58. Edward E. David and Frank Press, “Report to Malcolm Gillis by WAG Concerning Technology Transfer at Rice University,” 6 Jan. 1998, Rice University Assistant to the President papers: Carl MacDowell, 1963–2001, Woodson Research Center, Fondren Library, Rice University, Box 135, Folder 2.

59. E-mail from Daryl Boudreaux to Malcolm Gillis et al., “Brief Update to IP Advisory Board,” 17 Sept. 1999, Rice University President’s Office Records: President Malcolm Gillis 1993–2004 (UA 100), Rice University Archives, Woodson Research Center, Fondren Library, Rice University, Box 144, Folder 5.

60. Office of Technology Transfer, “Activity and Financial Summary,” 12 Oct. 2000, Rice University President’s Office Records: President Malcolm Gillis 1993–2004 (UA 100), Rice University Archives, Woodson Research Center, Fondren Library, Rice University, Box 144, Folder 5.

61. CCN proposal.

62. "Q&A with Richard Smalley," *Business Week*, August 24–31, 1998.

63. Kenneth Chang, "IBM Creates a Tiny Circuit out of Carbon," *New York Times*, August 27, 2001.

64. "Unidym Announces a Demonstration by Samsung Electronics of the First Color Carbon Nanotube-Based Electrophoretic Display," October 16, 2008 (http://www.unidym.com/press/pr_081016.html).

65. David Hecht, "Transparent Carbon Nanotube Films Likely Successor to ITO [indium-tin-oxide] for Commercial Applications," Phys.org, April 10, 2009 (http://phys.org/news158587561.html).

66. "Arrowhead Subsidiary Unidym, Inc. Acquired by Wisepower Co., Ltd. for up to $140 million earn-out plus $5 million royalties," Arrowhead Research Corporation, January 18, 2011 (http://www.arrowheadresearch.com/press-releases/arrowhead-subsidiary-unidym-inc-acquired-wisepower-co-ltd-140-million-earn-out-plus-5).

67. "Case Studies: Entegris' Advanced, Next Generation Reticle SMIF Pod," Unidym (http://www.unidym.com/products/casestudies_entegris.html). Note that there are only two "case studies" presented on Unidym's site. The other deals with coatings for the Tesla electric vehicle.

68. Based on conference programs found in the Richard E. Smalley papers, Chemical Heritage Foundation Collections, Box/Folder: 34/12; 39/7; 38/18; 39/1; 38/13; 38/9; 37/14.

69. On the controversy involving the Yale—Rice—Penn State team and the HP-UCLA team see Alfred Nordmann, "Molecular Disjunctions: Staking Claims at the Nanoscale," in *Discovering the Nanoscale*, ed. D. Baird, A. Nordmann, and J. Schummer (IOS Press, 2004). On the ontological indifference of nanotechnology see Nordmann, "Science in the Context of Technology," in *Science in the Context of Application*, ed. M. Carrier and A. Nordmann (Springer, 2011).

70. Eugenie Samuel Reich, *Plastic Fantastic: How the Biggest Fraud in Physics Shook the Scientific World* (Palgrave Macmillan, 2009).

71. Kenneth Chang, "Precursor to Tiniest Chip Is Developed," *New York Times*, October 18, 2001.

72. Leonard Cassuto, "Big Trouble in the World of 'Big Physics,'" *The Guardian* September 18, 2002.

73. Matt Crenson, "Chips Experiments Altered," *Victoria Advocate* [Associated Press syndicated article], October 4, 2002.

74. Doug Natelson, "This week in cond-mat," Nanoscale Views blog, April 26, 2007 (http://nanoscale.blogspot.com/2007/04/this-week-in-cond-mat_26.html).

75. Doug Natelson, “2010 Physics Nobel for graphene,” Nanoscale Views blog, October 5, 2010 (http://nanoscale.blogspot.com/2010/10/2010-physics-nobel-for-graphene.html).

76. Joerg Heber, “Great, the physics Nobel Prize for graphene! Now don’t overhype it …, ” All That Matters blog, October 5, 2010 (http://allthatmatters.heber.org/2010/10/05/great-the-physics-nobel-prize-for-graphene-now-dont-overhype-it/).

77. The quoted topics are from the websites of Paul McEuen (http://www.lassp.cornell.edu/lassp_data/mceuen/homepage/research.html) and Charles Lieber (http://cml.harvard.edu/), but similar biological topics appear in the portfolios of Mark Ratner, Mark Reed, and others. Other examples of Moletronics fundees moving into the life sciences include Anton V. Liopo et al., “Biocompatibility of native and functionalized single-walled carbon nanotubes for neuronal interface,” *Journal of Nanoscience & Nanotechnology* 6.5 (2006): 1365–1374 and Paul Alivisatos et al., “Nanotools for Neuroscience and Brain Mapping,” *ACS Nano* 7.3 (2013): 1850–1866.

78. Jim Tour began calling attention to homeland security issues as early as 2000, and has continued to publish and testify before Congress on this topic ever since. See James M. Tour, “Do-It-Yourself Chemical Weapons,” *Chemical & Engineering News* 78.28 (2000): 42–45.

79. PowerPoint slides for CBEN presentation, circa May 15, 2001, in Richard E. Smalley papers, Chemical Heritage Foundation Collections, Box 36, Folder 22.

80. Richard E. Smalley, “Of Chemistry, Love, and Nanobots,” *Scientific American* 285 (2001): 76–77; “Nanotechnology: Drexler and Smalley Make the Case For and Against ‘Molecular Assemblers,’” *Chemical & Engineering News* 81.48 (2003): 37–42.

81. Kaplan and Radin’s “Bounding an Emerging Technology” is the best study of the debate thus far.

82. For example, in the CBEN, programs for “Outreach to Underrepresented Groups” and “Intense Teacher Residency in Urban High School,” CBEN PowerPoint presentations for site visit to NSF, May 2001, in Richard E. Smalley papers, Chemical Heritage Foundation Collections, Box 36, Folder 22. The CCN proposal contained similar modules: “Educational outreach activities,” “Teacher training program,” and “Museum project.” Richard Smalley (PI), Proposal for Center for Carbon Nanotechnology, Richard E. Smalley papers, Chemical Heritage Foundation Collections, Box 31, Folder 5.

83. On the CBEN, see Elise McCarthy and Christopher Kelty, “Responsibility and Nanotechnology,” *Social Studies of Science* 40 (2010): 405–432; Christopher M. Kelty, “Beyond Implications and Applications: The Story of ‘Safety by Design,’” *NanoEthics* 3 (2009): 79–96. For European and North American perspectives on talk of “responsibility” in nanoscience, see Kamilla Lein Kjølberg and Roger Strand, “Conversations about Responsible Nanotech,” *NanoEthics* 5 (2011): 99–113 and Rider W. Foley, Ira

Bennett, and Jameson M. Wetmore, "Practitioners' Views on Responsibility: Applying Nanoethics," *NanoEthics* 6 (2012): 231–241.

84. See, e.g., Daniel Patrick Thurs, "Tiny Tech, Transcendent Tech: Nanotechnology, Science Fiction, and the Limits of Modern Science Talk," *Science Communication* 29 (2007): 65–95; Rutger van Merkerk and Harro van Lente, "Tracing Emerging Irreversibilities in Emerging Technologies: The Case of Nanotubes," *Technological Forecasting and Social Change* 72 (2005): 1094–1111; Terre Satterfield, Joe Conti, Barbara Herr Harthorn, Nick Pidgeon, and Anton Pitts, "Understanding Shifting Perceptions of Nanotechnologies and Their Implications for Policy Dialogues about Emerging Technologies," *Science and Public Policy* 40 (2013): 247–260; Dónal P. O'Mathúna, *Nanoethics: Big Ethical Issues with Small Technology* (Continuum, 2009).

Epilogue

1. On the turn toward economic justification for funding research, see Elizabeth Popp Berman, "Not Just Neoliberalism: Economization in US Science and Technology Policy," *Science, Technology & Human Values* 39.3 (2014): 397–431. On growing calls for accountable return on investment (in financial terms) in both government and corporate funding for research, see Glen Ross Asner, The Cold War and American Industrial Research, PhD dissertation, Carnegie Mellon University, 2006. Accountability was about much more than just money, though. The growth of a more generalized kind of accountability is documented in *Audit Culture: Anthropological Studies in Accountability, Ethics, and the Academy*, ed. Marilyn Strathern (Routledge, 2000).

2. For more on the use of centers to address workforce and representation issues, see Amy E. Slaton, *Race, Rigor, and Selectivity in U.S. Engineering: The History of an Occupational Color Line* (Harvard University Press, 2010); Juan C. Lucena, *Defending the Nation: U.S. Policymaking to Create Scientists and Engineers from Sputnik to the 'War on Terrorism'* (University Press of America, 2005).

Index

Air Force, US, 29, 81, 83, 87, 93, 97, 120, 191
Anacker, Wilhelm, 54, 56, 61, 65–67
Army, US, 25, 27, 31, 35, 36, 81, 191
AT&T, 50, 59, 124, 143, 151, 152, 162
Aviram, Arieh, 20, 91–103, 109, 111, 116, 185, 188–192, 210, 213–216

Ballantyne, Joseph, 133, 138, 143, 144
Bellcore, 152, 163, 169
Bell Labs, 27, 49, 50, 55, 56, 59, 86, 108, 122–127, 132, 143, 146, 152, 162, 163, 169, 174, 186, 213
Biology, 4, 9, 10, 94, 102–105, 109, 110, 114, 115, 121, 132, 168, 180, 171–178, 209, 216, 217, 223. *See also* Life sciences
Biotechnology, 4, 40, 102–105, 113, 114, 170, 173–177, 183, 216, 222–225
Bipolar junction transistors, 25, 30, 61, 92
Bliss, James, 29, 40, 41
Bloch, Erich, 18–21, 143, 160, 211
Buckminsterfullerene. *See* C_{60}
Buckytubes. *See* Carbon nanotubes

C_{60}, 187, 198–208
California Institute of Technology, 5, 59, 96, 98, 137, 138, 154–156
Carbon nanotubes, 85, 194, 204, 205, 208–212, 215
Carter, Forrest, 20, 21, 96–116, 128, 129, 136, 185–190, 195, 198, 213–216
Center for Integrated Systems, 18, 19, 22, 134, 135, 143, 147, 157–161, 164–167, 171–176, 183
Chandross, Edwin, 108, 114, 198
Charge-transfer salts, 85, 86, 89, 92, 215. *See also* TTF-TCNQ
Chemical & Engineering News, 102, 107, 108, 139, 217
Civilianization of research and markets, 1–3, 16–20, 23–43, 48, 75, 120, 121, 164, 167, 173, 221, 222, 225
Clean rooms, 6, 115, 123, 124, 134, 135, 158, 182
Cold War, 1, 2, 13–19, 22, 26, 29, 36, 38, 46, 50, 54, 75, 76, 79, 115, 117, 135, 149, 163, 164, 167, 183, 187–190, 207, 221, 222
Conducting polymers, 79, 88–91, 95–100, 112, 115, 120, 215
Conferences, 14, 28, 83–86, 99–112, 115, 122, 125–137, 162–165, 170, 175, 176, 189, 190, 193–195, 200, 203, 206, 212, 213
Cornell Nanofabrication Facility, 166, 216. *See also* National Submicron Facility

Cornell University, 21, 22, 73, 74, 87, 88, 100, 123, 131–146, 156–159, 163, 166–171, 174–182, 186, 208, 216, 223
Craighead, Harold, 163, 168–171, 175
Cryotron, 51–55, 61
Curl, Robert, 200, 204, 205

Defense Advanced Research Projects Agency, 26, 87, 108, 120, 135, 136, 145, 154, 159, 171, 180, 191–195, 207, 209, 212–216, 224
Drexler, K. Eric, 104–106, 116, 137, 206–208, 217, 218
Durham University, 112, 114, 115

Electron-beam lithography, 51, 59, 91, 100, 122–127, 131, 135, 138, 141–143, 162, 174, 175, 186, 187, 190
Engineering Research Centers, 144, 146, 147, 154, 160, 180
Engineering sciences, 16, 22, 33, 37, 38, 41, 54, 57, 127, 134, 167, 170, 216
Epochal break, 2, 14, 93
Everhart, Thomas, 122, 124, 131–133, 136, 140

Fairchild Semiconductor, 8, 12, 30, 44, 51, 52, 149, 160, 161, 165
Feynman, Richard, 59, 82, 96, 97, 137, 141, 174
Ford Motor Company, 48, 50, 51, 55, 56
Foundries, 6, 112, 165, 166

Gallium arsenide, 60, 68, 74, 138, 186, 201
Garito, Anthony, 86, 111
General Electric, 48–52, 55, 56, 83, 138, 143
Global economic competitiveness, 2–4, 10–15, 19, 74, 79, 99, 103, 121, 125–130, 143, 149, 150, 153, 160, 161, 164–167, 180, 199, 222
Gomory, Ralph, 62–67
Gordon Micro/Nanofabrication Conference, 128–131, 162, 163, 175, 176

Harris, Jay, 126–136, 145
Harvard University, 182, 194, 209, 214
Heath, Jim, 194, 196, 201, 209, 213–216
Heeger, Alan, 86–89, 96, 100, 111, 136
Hewlett-Packard, 44, 45, 135, 137, 155, 161, 194–198, 204, 213
Hughes Research, 122, 126, 130, 133, 138, 169
Human Genome Project, 173, 180, 181
Humanities, 41, 172, 182, 224

IBM. *See* International Business Machines
Industrial affiliates programs, 38, 135, 143, 156–160, 165–167, 171, 172, 176
Industrial consortia, 3, 6, 13, 19, 22, 23, 150–167, 172–175, 179, 180, 223, 224
Institute of Electrical and Electronics Engineers, 28, 59, 122
Integrated Circuits Laboratory, 29–32, 39, 41, 44, 45, 133, 134, 165, 171–173, 176–178, 181
Intel, 8, 12, 20, 31, 32, 39, 59, 60, 69, 94, 124, 125, 151, 154, 155, 161, 224
Interdisciplinarity, 3, 17, 18, 34, 40–44, 87, 88, 119–121, 127, 132–134, 144–147, 151, 167–172, 178, 183, 188, 199, 218, 222–225
Interdisciplinary Laboratories, 87, 88, 120, 135. *See also* Materials Research Laboratories
International Business Machines, 12, 14, 20, 21, 28, 31, 48, 50–76, 82, 83, 91–95, 98, 111, 119, 123, 124, 128, 129, 132, 137, 143, 146, 151, 152, 155, 162, 163, 186–189, 204, 205, 209, 211, 224

International Technology Roadmap for Semiconductors, 8, 72, 80, 164
Isaacson, Michael, 100, 140, 141, 174, 186

Japan, 2–4, 19, 44, 48, 74, 79, 84, 88, 99, 110–113, 117, 121, 125–128, 131, 136, 149–154, 161, 164, 167, 172, 180, 186–189, 222
Josephson, Brian, 49–55, 106
Josephson junctions, 48, 53–76, 82, 89, 91, 94, 99, 123, 124, 155, 185–187, 190, 211, 213

Kelly, Kevin (futurist and editor), 8, 10
Kelly, Kevin (physicist), 195, 207
Keyes, Robert W., 58–61, 64, 68, 94, 128–132, 136, 189, 190
Kleinsasser, Alan, 69, 123
Krumhansl, James, 135–137

Landauer, Rolf, 57, 58, 61, 70, 94
Lane, Neal, 202, 205, 208
Leslie, Stuart W., 23, 33, 50, 156, 157
Lieber, Charles, 194, 209, 213, 214
Life sciences, 3, 4, 22, 23, 37, 40, 94, 120, 146, 167–178, 182, 183, 209, 216–219, 224. *See also* Biology
Limits to scaling, 11, 57–60, 80–83, 93, 94, 99, 110, 113, 124, 128, 140, 141, 171, 174, 189
Lincoln Laboratory, 131–133, 162, 163
Linear model of innovation, 21, 26, 36, 54, 55, 64, 72, 76
Linvill, John, 27–45, 75, 134, 135, 164–167, 170–173
Logue, Joseph, 65–68

MacDiarmid, Alan, 87–89, 96, 100
Manufacturing, 1, 4–8, 11–15, 19, 20, 31, 41, 50, 53–57, 60–70, 74, 89, 91, 103, 105, 116, 121–126, 133, 149–152, 159–167, 171–175, 183, 187, 190, 210, 212, 222–224
Massachusetts Institute of Technology, 51, 82, 88, 120, 131–138, 141, 143, 146, 156, 162, 163, 166, 178, 179, 182, 190
Materials Research Laboratories, 120, 131–133, 136, 140, 154, 180, 182, 224. *See also* Interdiscicplinary Laboratories; Materials Research Science and Engineering Centers
Materials Research Science and Engineering Centers, 144, 182. *See also* Materials Research Laboratories
Matisoo, Juri, 53–56, 63, 67, 71, 72
Mead, Carver, 59, 60, 136, 154–156
Meindl, James, 18, 20, 25–45, 60, 134–138, 170–172, 176
Metal-oxide-semiconductor field-effect transistors, 12, 20, 25, 27, 30–36, 39, 42, 43, 60, 72, 132
Metzger, Robert, 109, 195
Microelectronics and Computer Technology Corporation, 152, 153, 157
Military, US, 1, 2, 16, 25–41, 48, 51, 54, 61, 75, 81–83, 87, 99, 120, 121, 127, 136, 149, 164–167, 172, 173, 191, 195, 221–225
Ministry of International Trade and Industry, 74, 75, 110, 111, 126, 150, 151, 156, 186
Mission-oriented funding agencies, 26, 28, 35, 36, 39, 61, 88, 166
Molecular-beam epitaxy, 60, 138, 162, 186, 187
Moletronics program, 190, 193, 194, 207, 209, 212–216
Moore, Gordon, 5, 7, 10, 12, 15, 20, 31, 46, 53, 136, 143–145, 154, 160, 166
Moore's Law, 4–15, 19–23, 31, 52, 53, 59–61, 64, 65, 72, 79–81, 93, 94, 99, 113, 121, 125, 134, 146, 147, 151, 155, 165, 171, 179, 189, 198–200, 203, 207, 211, 212, 219, 221, 225

Nanotechnology, 22, 59, 71, 82, 104, 116, 137, 145, 158, 171, 175, 181, 182, 187, 194, 198, 199, 202, 205–219, 223
Natelson, Doug, 207, 214, 215
National Aeronautics and Space Administration, 26, 29, 30, 34, 36, 61, 171, 172, 213
National Geographic, 97, 139, 185–187
National Laboratories, 13, 138, 197, 224
National Nanofabrication Facility, 146, 167, 168, 170, 175–177. *See also* National Submicron Facility
National Nanofabrication Users Network, 22, 178–182, 208, 223. *See also* National Nanotechnology Infrastructure Network
National Nanotechnology Infrastructure Network, 181–183, 187. *See also* National Nanofabrication Users Network
National Nanotechnology Initiative, 137, 181, 182, 187, 198, 199, 202, 212, 213, 217, 218
National Research and Resource Facility for Submicron Structures, 21, 22, 73, 100, 126–145, 165, 168, 174, 181–183, 186, 187. *See also* National Submicron Facility
National Science Foundation, 18, 22, 30, 36, 37, 73, 107, 120–122, 125–127, 131–147, 159, 160, 171, 176–182, 202, 203, 205, 208–213, 216, 217, 222, 224
National Security Agency, 26, 51, 57, 61, 62, 68, 73, 75, 76, 108
National-security state, 1, 2, 16, 18, 21, 22, 26, 29, 34–37, 48, 68, 73, 76, 79, 83, 149, 221
National Submicron Facility, 131, 136–140, 143–147, 150, 156, 157, 159, 168–170, 174, 176, 180. *See also* National Research and Resource Facility for Submicron Structures; National Nanofabrication Facility; Cornell Nanofabrication Facility
Naval Research Laboratory, 20, 96, 98–102, 107, 108, 112, 186
Navy, US, 28, 29, 34, 73, 74, 81, 96, 100, 107, 171, 185
Network failures, 21, 75, 76, 79, 88, 108, 128, 137, 187, 188, 193
Nobel Prizes, 50, 55, 71, 72, 89, 198, 204, 215
Northwestern University, 87, 95, 194
Noyce, Robert, 8, 31, 45, 51, 59, 124, 127, 161

Office of Education, US, 30, 43, 44, 171
Office of Naval Research, 28–30, 73, 74, 88, 96, 100, 191, 210
Optacon, 27–35, 41–46, 165, 172
Optical lithography, 59, 60, 82, 101, 122–126, 174, 175
Organic conductors, 79, 85–96, 111, 115, 190, 223

Para-scientific media, 102, 107, 137
Patents and intellectual property, 2–4, 12, 16, 25, 40, 41, 45, 52, 55, 62, 91, 95, 139, 152, 155, 169, 174, 188, 197, 210, 211
Pease, Fabian, 122, 124, 133, 141, 142, 163, 173–173, 190
Pennsylvania State University, 179, 181, 182, 194, 195, 197, 198, 213
Philanthropic foundations, 13–16, 36, 43, 119, 135, 173, 221, 225
Power consumption, 9, 20, 27, 31, 32, 35, 36, 39, 47, 60, 61, 64, 82, 110
Pugh, Emerson, 64, 65, 68

Ratner, Mark, 92–103, 106, 109, 116, 188, 194, 213–216
RCA, 12, 31, 50, 83, 132, 151, 152

Rectifiers, 27, 92–95, 98, 106, 109, 189, 190
Reed, Mark, 100, 106, 190–194, 197, 198, 207, 209, 215
Rhodopsin, 109, 110, 216
Rice Quantum Institute, 199, 200, 203–207, 223
Rice University, 187, 194–218, 223
Roberts, Gareth, 111–115

Scanning tunneling microscope, 71, 106, 188, 189, 194
Science, 4, 6, 45, 107, 139, 185, 195–198
Science and Technology Centers, 144, 180, 203, 209, 216
Science policy, 3, 4, 19, 21, 26, 44, 46, 50, 76, 79, 112, 115, 135, 143–150, 153, 160–164, 176, 180, 183–187, 212, 213
Seiden, Philip, 53, 91, 92, 95, 103, 111
SEMATECH, 151–153, 157, 158, 167, 175, 211
Silicon Structures Project, 137, 154–156
Silicon Valley, 14, 20, 42–46, 59–61, 67, 72, 94, 125, 127, 151, 152, 160, 161, 173, 174, 185, 194
Smalley, Richard E., 187, 188, 194, 195, 198–213, 216–219
Smith, Henry I., 131–133, 136, 141, 190
Smith, Ken, 203, 207, 210
Social fact, 4, 9, 10, 52, 53, 75, 125
Social sciences, 36, 37, 120, 172, 180, 182, 224
Soviet Union, 48, 73, 76, 83, 111, 149, 164, 188
Sputnik, 26, 33, 83, 87, 222
Stanford Research Institute, 29, 35, 41, 44
Stanford University, 18, 20, 22, 25–45, 48, 75, 78, 119, 121, 133–138, 141–143, 146, 156–159, 162, 163, 167, 171–182, 190, 205, 208, 222, 223
Start-up companies, 3, 4, 8, 12–16, 31, 41, 60, 103, 105, 151, 158, 159, 170, 183, 210, 216
Student activism and protest, 3, 20, 33–39, 43, 119
Submicrometer Structures Laboratory, 133, 138, 147

Texas Instruments, 51, 101, 133, 138, 151, 190, 191, 201–204
Thin films, 49, 53, 55, 56, 71, 95, 112, 115, 124, 136, 186, 197
Three Beams meeting, 122, 125, 129–131
Tour, James, 190–198, 209, 210, 214–216
TTF-TCNQ, 85–92, 111. *See also* Charge-transfer salts

University of California, Berkeley, 5, 122, 131–133, 182, 205
University of California, Los Angeles, 194–198, 209, 213, 214
University of California, Santa Barbara, 163, 179, 181
University of Minnesota, 137, 138, 159
University of Pennsylvania, 55, 86–89, 96, 120, 127, 128, 131, 132, 168, 209, 223
University of South Carolina, 190, 209, 210
University of Texas, 138, 157, 158
University of Washington, 126, 158, 182, 223

Vacuum tubes, 6, 10, 51, 63, 65, 84, 93
Very Large Scale Integration, 44, 79, 99, 110, 123, 126, 127, 131, 150–152, 156, 174

Washington University, 127, 128, 182
Weiss, Paul, 194–198

Westinghouse, 27, 50, 82–84, 87, 93, 97, 98, 112
Williams, Stan, 194, 196, 198, 207, 213, 215
Wolf, Edward D., 122–124, 127, 130–137, 140, 141, 143, 168–171, 174, 175
Wynne, Kenneth, 88, 89, 96, 100

Xerox, 91, 111, 112, 135, 143, 154, 155
X-ray lithography, 60, 122–125, 131, 141, 175, 190

Yale University, 100, 101, 137, 190, 195–198, 213

Zig-zag model of innovation, 21, 48, 54–56, 71, 72, 76, 77, 91, 175, 224

Inside Technology

edited by Wiebe E. Bijker, W. Bernard Carlson, and Trevor Pinch

Cyrus C. M. Mody, *The Long Arm of Moore's Law: Microelectronics and American Science*

Harry Collins, Robert Evans, and Christopher Higgins, *Justice, Accuracy, and Uncertainty: Technology's Attack on Umpires and Referees and How to Fix It*

Tiago Saraiva, *Fascist Pigs: Technoscientific Organisms and the History of Fascism*

Teun Zuiderent-Jerak, *Situated Intervention: Sociological Experiments in Health Care*

Basile Zimmermann, *Technology and Cultural Difference: Electronic Music Devices, Social Networking Sites, and Computer Encodings in Contemporary China*

Andrew J. Nelson, *The Sound of Innovation: Stanford and the Computer Music Revolution*

Sonja D. Schmid, *Producing Power: The Pre-Chernobyl History of the Soviet Nuclear Industry*

Casey O'Donnell, *Developer's Dilemma: The Secret World of Videogame Creators*

Christina Dunbar-Hester, *Low Power to the People: Pirates, Protest, and Politics in FM Radio Activism*

Eden Medina, Ivan da Costa Marques, and Christina Holmes, editors, *Beyond Imported Magic: Essays on Science, Technology, and Society in Latin America*

Anique Hommels, Jessica Mesman, and Wiebe E. Bijker, editors, *Vulnerability in Technological Cultures: New Directions in Research and Governance*

Amit Prasad, *Imperial Technoscience: Transnational Histories of MRI in the United States, Britain, and India*

Charis Thompson, *Good Science: The Ethical Choreography of Stem Cell Research*

Tarleton Gillespie, Pablo J. Boczkowski, and Kirsten A. Foot, editors, *Media Technologies: Essays on Communication, Materiality, and Society*

Catelijne Coopmans, Janet Vertesi, Michael Lynch, and Steve Woolgar, editors, *Representation in Scientific Practice Revisited*

Rebecca Slayton, *Arguments that Count: Physics, Computing, and Missile Defense, 1949–2012*

Stathis Arapostathis and Graeme Gooday, *Patently Contestable: Electrical Technologies and Inventor Identities on Trial in Britain*

Jens Lachmund, *Greening Berlin: The Co-Production of Science, Politics, and Urban Nature*

Chikako Takeshita, *The Global Biopolitics of the IUD: How Science Constructs Contraceptive Users and Women's Bodies*

Cyrus C. M. Mody, *Instrumental Community: Probe Microscopy and the Path to Nanotechnology*

Morana Alač, *Handling Digital Brains: A Laboratory Study of Multimodal Semiotic Interaction in the Age of Computers*

Gabrielle Hecht, editor, *Entangled Geographies: Empire and Technopolitics in the Global Cold War*

Michael E. Gorman, editor, *Trading Zones and Interactional Expertise: Creating New Kinds of Collaboration*

Matthias Gross, *Ignorance and Surprise: Science, Society, and Ecological Design*

Andrew Feenberg, *Between Reason and Experience: Essays in Technology and Modernity*

Wiebe E. Bijker, Roland Bal, and Ruud Hendricks, *The Paradox of Scientific Authority: The Role of Scientific Advice in Democracies*

Park Doing, *Velvet Revolution at the Synchrotron: Biology, Physics, and Change in Science*

Gabrielle Hecht, *The Radiance of France: Nuclear Power and National Identity after World War II*

Richard Rottenburg, *Far-Fetched Facts: A Parable of Development Aid*

Michel Callon, Pierre Lascoumes, and Yannick Barthe, *Acting in an Uncertain World: An Essay on Technical Democracy*

Ruth Oldenziel and Karin Zachmann, editors, *Cold War Kitchen: Americanization, Technology, and European Users*

Deborah G. Johnson and Jameson W. Wetmore, editors, *Technology and Society: Building Our Sociotechnical Future*

Trevor Pinch and Richard Swedberg, editors, *Living in a Material World: Economic Sociology Meets Science and Technology Studies*

Christopher R. Henke, *Cultivating Science, Harvesting Power: Science and Industrial Agriculture in California*

Helga Nowotny, *Insatiable Curiosity: Innovation in a Fragile Future*

Karin Bijsterveld, *Mechanical Sound: Technology, Culture, and Public Problems of Noise in the Twentieth Century*

Peter D. Norton, *Fighting Traffic: The Dawn of the Motor Age in the American City*

Joshua M. Greenberg, *From Betamax to Blockbuster: Video Stores and the Invention of Movies on Video*

Mikael Hård and Thomas J. Misa, editors, *Urban Machinery: Inside Modern European Cities*

Christine Hine, *Systematics as Cyberscience: Computers, Change, and Continuity in Science*

Wesley Shrum, Joel Genuth, and Ivan Chompalov, *Structures of Scientific Collaboration*

Shobita Parthasarathy, *Building Genetic Medicine: Breast Cancer, Technology, and the Comparative Politics of Health Care*

Kristen Haring, *Ham Radio's Technical Culture*

Atsushi Akera, *Calculating a Natural World: Scientists, Engineers and Computers during the Rise of US Cold War Research*

Donald MacKenzie, *An Engine, Not a Camera: How Financial Models Shape Markets*

Geoffrey C. Bowker, *Memory Practices in the Sciences*

Christophe Lécuyer, *Making Silicon Valley: Innovation and the Growth of High Tech, 1930–1970*

Anique Hommels, *Unbuilding Cities: Obduracy in Urban Sociotechnical Change*

David Kaiser, editor, *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives*

Charis Thompson, *Making Parents: The Ontological Choreography of Reproductive Technology*

Pablo J. Boczkowski, *Digitizing the News: Innovation in Online Newspapers*

Dominique Vinck, editor, *Everyday Engineering: An Ethnography of Design and Innovation*

Nelly Oudshoorn and Trevor Pinch, editors, *How Users Matter: The Co-Construction of Users and Technology*

Peter Keating and Alberto Cambrosio, *Biomedical Platforms: Realigning the Normal and the Pathological in Late-Twentieth-Century Medicine*

Paul Rosen, *Framing Production: Technology, Culture, and Change in the British Bicycle Industry*

Maggie Mort, *Building the Trident Network: A Study of the Enrollment of People, Knowledge, and Machines*

Donald MacKenzie, *Mechanizing Proof: Computing, Risk, and Trust*

Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences*

Charles Bazerman, *The Languages of Edison's Light*

Janet Abbate, *Inventing the Internet*

Herbert Gottweis, *Governing Molecules: The Discursive Politics of Genetic Engineering in Europe and the United States*

Kathryn Henderson, *On Line and On Paper: Visual Representation, Visual Culture, and Computer Graphics in Design Engineering*

Susanne K. Schmidt and Raymund Werle, *Coordinating Technology: Studies in the International Standardization of Telecommunications*

Marc Berg, *Rationalizing Medical Work: Decision-Support Techniques and Medical Practices*

Eda Kranakis, *Constructing a Bridge: An Exploration of Engineering Culture, Design, and Research in Nineteenth-Century France and America*

Paul N. Edwards, *The Closed World: Computers and the Politics of Discourse in Cold War America*

Donald MacKenzie, *Knowing Machines: Essays on Technical Change*

Wiebe E. Bijker, *Of Bicycles, Bakelites, and Bulbs: Toward a Theory of Sociotechnical Change*

Louis L. Bucciarelli, *Designing Engineers*

Geoffrey C. Bowker, *Science on the Run: Information Management and Industrial Geophysics at Schlumberger, 1920–1940*

Wiebe E. Bijker and John Law, editors, *Shaping Technology / Building Society: Studies in Sociotechnical Change*

Stuart Blume, *Insight and Industry: On the Dynamics of Technological Change in Medicine*

Donald MacKenzie, *Inventing Accuracy: A Historical Sociology of Nuclear Missile Guidance*

Pamela E. Mack, *Viewing the Earth: The Social Construction of the Landsat Satellite System*

H. M. Collins, *Artificial Experts: Social Knowledge and Intelligent Machines*

http://mitpress.mit.edu/books/series/inside-technology